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A History of psychology in autobiography

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A HISTORY OF PSYCHOLOGY IN AUTOBIOGRAPHY

VOLUME II

JAN 25 1934

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VOLUME II

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Edited by
Carl Murchison

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PREFACE TO THE SERIES*

The author of a recent history of psychology found that it was impossible to get important facts concerning the scientific development of certain individuals except from those individuals themselves. Since a science separated from its history lacks direction and promises a future of uncertain importance, it is a matter of consequence to those who wish to understand psychology for those individuals who have greatly influenced contemporary psychology to put into print as much of their personal histories as bears on their professional careers.

The initial idea, which later developed into the general plan for *A History of Psychology in Autobiography,* was contained in a letter of April 10, 1928, from Edwin G. Boring to Carl Murchison. Shortly afterwards, there was a conference in Emerson Hall, participated in by Edwin G. Boring of Harvard University, Karl Bühler of the University of Vienna, and Carl Murchison of Clark University, which resulted in our inviting Herbert S. Langfeld of Princeton University and John B. Watson of New York City to join with us in a committee which would assume responsibility for all invitations extended for inclusion in such a series.

We then proceeded in the following manner in arriving at a tentative list of individuals to whom invitations were to be sent. Without consultation, each member of the Committee compiled a list of one hundred names that he considered eligible for such a series. The five lists of one hundred each were then combined, making a composite list of one hundred eighty-four names. Taking the one hundred eighty-four names as a nomination list, each member of the Committee then voted for sixty names. All chosen unanimously were placed upon a preferred list to whom invitations would be sent as the Series progressed.

Since preparing the original list, the members of the Committee, in conference, have expanded somewhat the principle of selection which originally guided them. We agreed that individuals on the fringe of psychology or even in a neighboring field of science might so influence psychology that they should be included.

It must not be thought that individuals were invited in order of

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***Reprinted from Volume I.**

eminence or of seniority exclusively. There are many factors which determine the order in which autobiographies appear in this Series and it is very probable that some of the most eminent individuals will appear in later volumes rather than in earlier ones. In two or three cases, we decided that certain individuals of the greatest eminence should probably not appear in the first one or two volumes because they are already well known to most psychologists and have recently been discussed at length in other histories, especially in Boring's *A History of Experimental Psychology*. During the next year, perhaps three volumes altogether will have appeared, while a fourth volume will appear in the near future. If our idea proves to be as fruitful as we now believe that it will, it is to be expected that additional volumes will appear at intervals of three or four years.

In this Series, the printing of long and comprehensive bibliographies has been definitely avoided, since these will be accessible in the *Psychological Register*.

CLARK UNIVERSITY
Worcester, Massachusetts
May 22, 1930

CARL MURCHISON, *Chairman*
EDWIN G. BORING
KARL BÜHLER
HERBERT S. LANGFELD
JOHN B. WATSON

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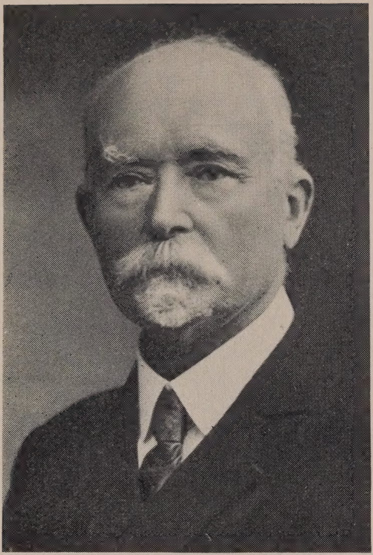

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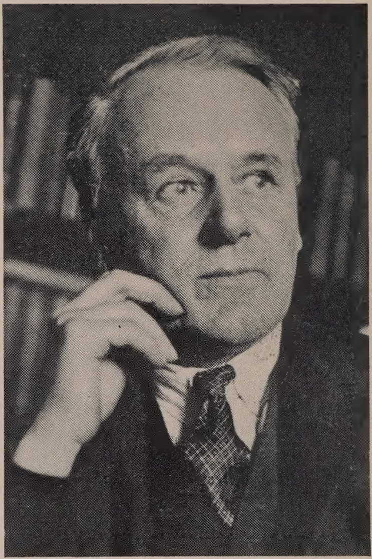

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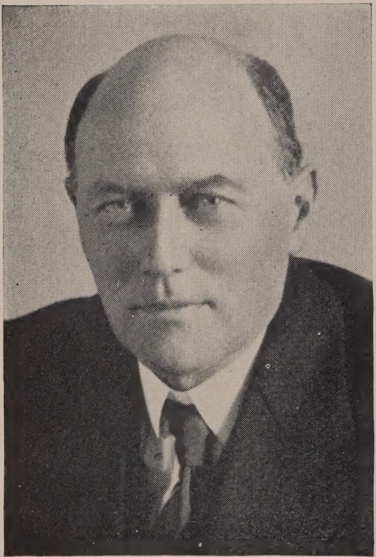

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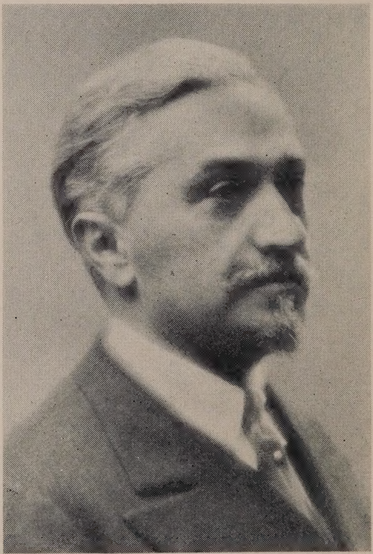

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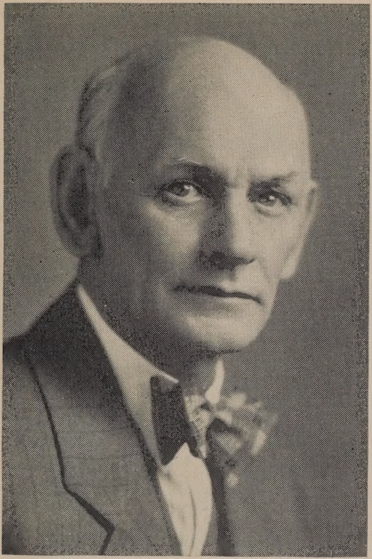

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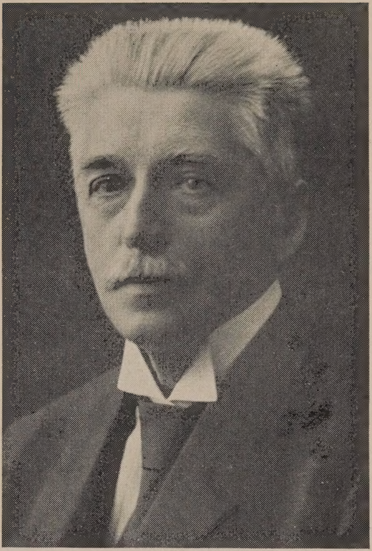

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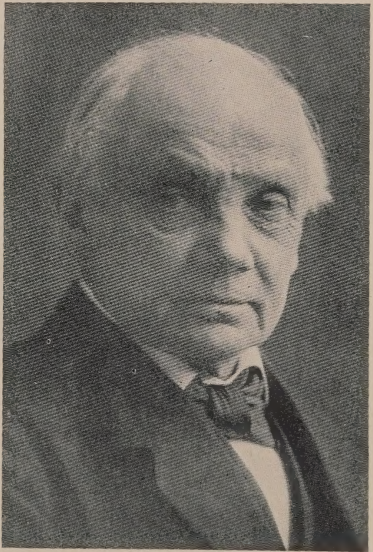

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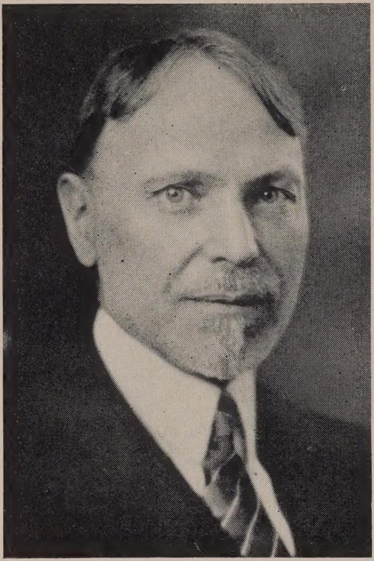

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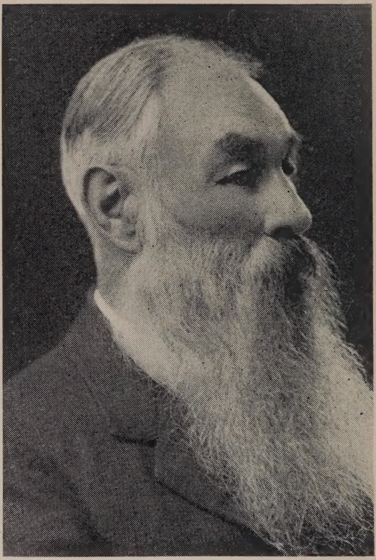

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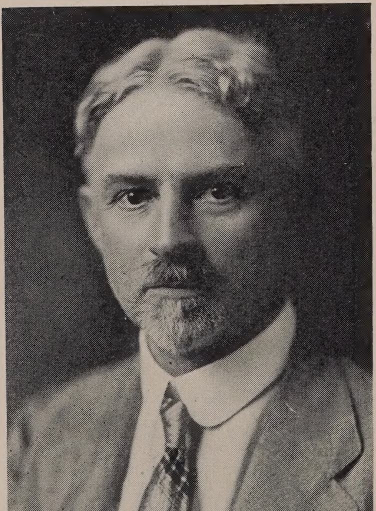

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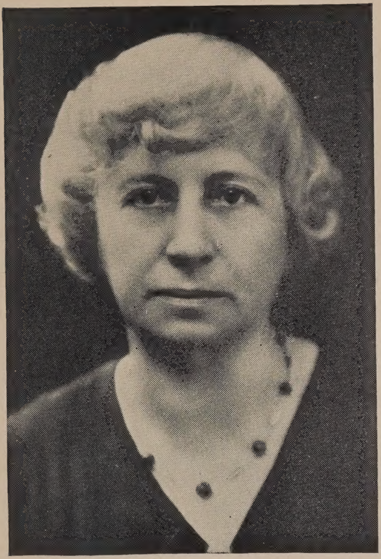

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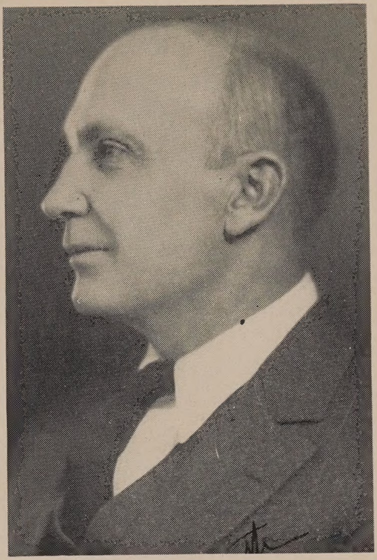

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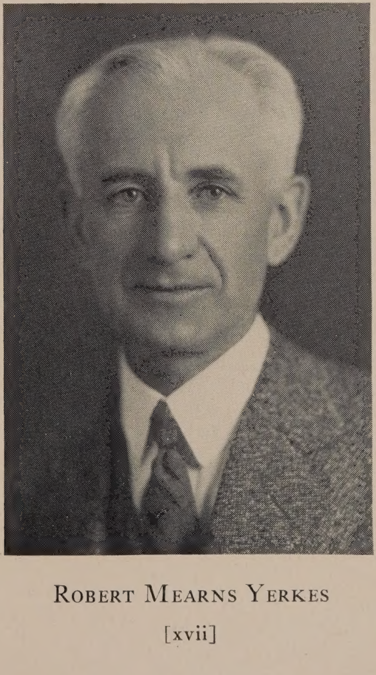

Robert Mearns Yerkes

A HISTORY OF PSYCHOLOGY IN AUTOBIOGRAPHY

VOLUME II

BENJAMIN BOURDON*

HEREDITY, ENVIRONMENT, AND EDUCATION

I was born in Normandy in 1860, in a village on the seacoast (Montmartin-sur-mer), of parents themselves of Norman origin. My father had no trade at the time of my birth, but when I was about eight years old he became a farmer, cultivating a small farm which he had inherited from his father and which he worked from then on for many years energetically and intelligently. He was the son and grandson of notaries; his mother was the daughter of a naval officer, whose life must have been rather adventurous—in fact, he had been nicknamed "the corsair." My mother was of humble origin; her father was a modest farmer, but at the same time a good mason, and he directed the construction of quite a few of the more important houses which were built in the town during his lifetime. I did not know my father's parents since they died at about the time I was born, but I did know my maternal grandparents, both of whom died at a very ripe old age, and I remember them very well for I saw a great deal of them. They were unassuming people, superior in mental ability to their position, as was my mother; unfavorable circumstances and the lack of ambition had doubtless kept them from reaching a social level superior to that in which they remained.

With the exception of five or six educated men, such as the priest, the notary, and the doctor, my village, which had a population of about one thousand, was made up of three main classes of people: peasants, who formed the main class, sailors, and quarry workers, the latter being employed in working the important calcareous deposits and numerous lime kilns which exist in the region. It was in this community, composed of farmers, sailors, and quarry workers, that I spent the whole of my childhood and a part of my adolescence. These people didn't worry about abstract ideas and were not satisfied with mere words. This environment has doubtless exerted a great influence upon my mental development.

At the age of twelve, I entered as a boarding student the lycée of Coutances, where, from the standpoint of scholastic achievement, I was a good pupil. I left there, at the age of nineteen, a *bachelier ès lettres et ès sciences*. In the lycée, as in the other lycées of my country, rhetoric was extensively cultivated at the time when I was there; I have always felt a great aversion for this rhetoric and I made a poor showing when it was a question in my themes of making Cicero, or some other character of ancient or modern times, speak. On the other hand, I displayed a very great interest, during my years at the lycée, in mathematical studies, languages, physics and chemistry, drawing, philosophy, and also in physical exercises, gymnastics. I certainly would have become interested in manual training if it had been taught; in fact, I have always been fond of that kind of work, and, during more recent years, I believe I have even used up too much time in constructing my own instruments or apparatus for my experiments.

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*Submitted in French and translated for the Clark University Press by Leland L. Atwood.

In the learned circles with which I came in contact during my adolescence, the professions which were especially esteemed were those for which the study of law prepared. When I left the lycée, I hesitated between a teaching career and the study of law. Having at first sought a teaching position, I was appointed teacher of mathematics in a small secondary school in Brittany. I did not accept, and, since I had found at Paris a position which allowed me to live there, and left me much leisure time, I decided to study law. So for a year I studied in the law school at Paris. I was fairly interested in criminal law and Roman law, but not so much so in French civil law: the different interpretations of the civil code which were given us by the professor or which I read in books often impressed me as hair-splitting, complicating disagreeably the study of this civil law.

I finally turned to preparation for a teaching career and decided upon the teaching of philosophy. For a few years I took at the *Faculté des Lettres* at Paris the philosophical courses of Paul Janet, Caro, Joly, and Carrau. The first three were *spiritualistes.* Carrau, who taught the history of philosophy, did not have, I believe, any well-defined personal theory. Janet, very old, gave his courses without paying the slightest attention to his students. We students had intimate relations with only the three other professors mentioned. Carrau was full of zeal, and we remember him well because of the care with which his lessons were prepared. On the other hand, Joly was evidently little interested in us. One of my memories relative to his courses was that of the following subject which he had asked us to discuss in writing: "Method in Metaphysics"; I gave him a dissertation whose conclusion was that the proper method in metaphysics was not to have any method; Joly advised me to avoid this conclusion if I had to treat the same subject in the examinations. Of the four professors mentioned, the one who probably had the strongest personality was Caro. The general public, basing its judgment on the social success of Caro and the works that he published, did not know him well; he was in reality a fighter, sensitive, intelligent to a high degree; but he was, to tell the truth, a victim of his social success, having doubtless preferred that to a more or less obscure serious labor. The meetings that we students had with him often passed in the following way: One of us would discuss for about three-quarters of an hour a certain question, and, for the remaining quarter of an hour, Caro would take the floor, giving his own opinion of the question and criticizing the student's presentation. Sometimes Caro would ask us to sum up some philosophical work that had recently appeared, which, according to some people, would enable him to speak, in his turn, of the work in his public courses or in his books, without having read it. Caro, during my last year at the Sorbonne, became rather fond of me and chose me as an intermediary between the other students and himself. One of the reasons for this inclination was, I believe, his combative disposition and the possibility I offered him of exercising it at my expense, expounding to him ideas which often were opposed to his own. Caro was, of all our professors, the one who, perhaps with the least personal effort, succeeded in getting the most work out of us; I mention this fact, because it seems to me to be most interesting pedagogically.

Along with courses of philosophy, during my years of study at Paris, I took various courses having no direct relation to philosophy. I shall mention especially those of the alienist Magnan at the St. Anne Asylum, those of Charcot at the Salpêtrière, and those of the physiologists Brown-Séquard and Franck at the Collège de France.

My intellectual development, I believe, was little influenced by the teaching of philosophy as it was given me when I was a student at the Sorbonne. The influences which had a strong impression upon me, doubtless because I was predisposed to undergo their influence, were those of certain of my readings. Berkeley, Hume, the Mills, Bain, Spencer, and James intensely interested me. Ribot contributed to my intellectual development by his books and also to some extent by his oral teaching. The *neo-criticiste* doctrine, represented by Renouvier, was flourishing at the time when I was a student; we used to discuss it with our fellow students and we read the *Critique philosophique,* edited by Renouvier, as well as the books of that philosopher. The influence exercised upon me by the *neo-criticiste* creed was, however, only slight; I recognized the serious nature of this creed, but the psychology of Renouvier seemed to me inadequate, too abstract, too dialectical, and too remote from observation. As regards Kant, whom I had to study, because of the examinations, my impression was nearly the same as that concerning Renouvier. As for the philosophers like Fichte, Schelling, Hegel, whose theories I also had to have some inkling of, I have never been able to surmount the aversion that they caused in me. I remember having purchased second-hand, very cheaply, a translation of one of Schelling's works, and thinking at first that I had made a good find; however, I was afterwards very happy, after having tried to read it, to find a fellow student who was willing to take over the volume for the price that I myself had paid.

After having successfully passed the examination for the degree of *Agrégé* in philosophy, I won a scholarship which allowed me to spend a year in Germany. I went at first to Heidelberg, where, during a first semester, I took, among others, the courses of the linguist Osthoff, who was then one of the best known of the *Junggrammatiker*. I hastened to become somewhat initiated in experimental psychology and to hear its illustrious representative, Wundt. I left then, at the beginning of the second semester to the school year, for Leipzig; I was supplied with a letter of introduction from Osthoff to his colleague Brugmann, like himself an eminent neo-grammarian, and with another from Ribot for Wundt. Brugmann and Wundt received me very kindly. I took the courses of both; moreover, I took part as an observer in the experimental research which went on in Wundt's laboratory.

Upon my return to France, I was appointed Professor of Philosophy, at first in the Lycée of Valenciennes, then, a year later, in that of Rennes. After having taught in the latter lycée for three years, I was appointed Assistant Professor of Philosophy at the University of Lille, where I remained a little more than a year. I returned finally to Rennes, where I soon won the rank of professor in the *Faculté des Lettres*.

TEACHING AND PUBLICATIONS

In France, in the important universities and also sometimes in the smaller universities, the students in the *Faculté des Lettres* are prepared for two kinds of examinations; some are taken in each university and the candidates have their own professors as examiners; when one has successfully passed these examinations, one receives the degree of *licencié*. The others are competitive among students (or any candidates) from all the universities who already have the degree of *licencié*; the oral part of these examinations takes place at Paris, and the examiners for both the written part and the oral part are generally professors other than those whose courses the candidates have taken. The candidates passing the latter examinations have the title of *agrégé*. The examinations for this degree form one example of that terrible centralization from which France suffers. Even in the case of examinations for the degree of *licencié*, this centralization makes itself felt: the programs of these examinations are made out for the most part by boards with their headquarters in Paris.

Under these conditions, original teaching is difficult in our universities. Moreover, a new difficulty results from the fact that the teaching called "higher," which our universities are supposed to give, is to a high degree controlled in reality by the so-called secondary instruction given in the *collèges* and *lycées*. Both the examinations for the degree of *licencié* and those for the degree of *agrégé* are made up essentially with a view to the training of teachers for the secondary schools.

Having begun my teaching in higher institutions at Lille, an important university center, I had to prepare the students there for the degrees of *licencié* and *agrégé*. Fortunately, my stay in that city was of short duration. In the less important University of Rennes, I was able to give up preparing students for the degree of *agrégé* and to remain, to a certain extent, master of my teaching. I profited from the relative independence which I thus enjoyed at Rennes in trying to organize there the teaching of experimental psychology and to found a psychological laboratory. Fortunately, the attitude of my colleagues toward my attempts was all that could be desired. When I arrived at Rennes, I had the especial good luck to find as Dean of the *Faculté des Lettres* the eminent Celtic scholar, Loth (he became later Professor of the Celtic Language and Literature at the Collège de France, Paris). Loth, hostile like myself to standardization and routine, champion in the cause of decentralization in universities, turned out to be very favorable to my plans and helped in obtaining for me the necessary rooms and funds for the founding of my laboratory. Later, after Loth had left, I found the same sympathetic attitude in the one who replaced him as dean, that other excellent Celtic scholar, Dottin. Loth and Dottin were both intensely interested, for personal reasons, in the founding of my laboratory; this laboratory was, even at the very first, also theirs to some extent. Both were linguists; their attention had been drawn to the researches of the celebrated phonetician, Rousselot, and it was their opinion that the laboratory should be devoted not only to psychology but also to phonetics. For some time, in fact, the laboratory was considered as a laboratory of experimental psychology and phonetics, and was called by this name. We entered into friendly relations with Rousselot, who gave freely of his advice, and we acquired most of the instruments that he himself had designed. Later, I gave over to one of my colleagues, P. Le Roux, the greater part of the apparatus relating to phonetics, and a laboratory of phonetics was established, separate from that time on from the psychological laboratory.

My first written work which seems worthy of consideration was prepared for a competition in which the question under discussion was the liberty of conscience. A fairly substantial sum was to reward the author of the winning essay. This prize was divided between two of the competitors, and I was one of these favored persons. The paper has never been published. It was entitled *Liberté et Religion.* The basic idea from which I took my departure was that the intolerance to be combatted develops under the influence of institutions (the State, religion, education, etc.), themselves intolerant, and that free thought will manifest itself where the institutions concerned are liberal. I had taken as my motto the words of Bacon: "Remove the cause and you remove the effect."[1]

In 1892, I received the doctor's degree, with a French thesis entitled *L'Expression des Émotions et des Tendances dans le Langage* and a Latin thesis discussing the qualities of perception in Descartes. The French thesis, which I wrote rather hastily, probably needs numerous corrections. I gave special consideration in it to the part that the intensity of the voice, its pitch, and the more or less great speed of speech may play as means of expressing our tendencies and feelings.

I have always been interested in the study of language and, more generally, of expression. Since my French thesis, I have published a few articles on the general problem of language, and I have many times given my students a course in expression, fairly well detailed. I have paid attention both to the normal psychology and the pathological psychology of language. During the last war, my laboratory having become a hospital, I had the opportunity of observing there soldiers who were wounded in the head and were suffering from aphasia; for almost an entire year, I endeavored, by daily exercises, to re-educate in the use of speech one of these wounded men who, in the beginning, presented an almost absolute aphasia; and I had the satisfaction, when he left Rennes, of knowing that he spoke well enough not to be embarrassed in his relations with his fellow men.

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[1] The competition in question was reviewed in a volume by Marillier (1890) entitled *La Liberté de Conscience.*

Through the numerous reviews that I have published in the *Revue Philosophique* and the *Année Psychologique,* I hope that I have contributed somewhat to bringing about a knowledge in France of studies in the German and English languages relating to experimental psychology. It was at my suggestion that Sanford's excellent manual was translated into French, and I myself reviewed the translation in rather careful detail. Sanford's work, which was very lucid, seemed to me especially adapted to initiate my countrymen in experimental psychology. The success of the work has not, perhaps, been as great as I had hoped, because of our university centralization and the fact that our administrators in philosophical and psychological studies have generally been interested in metaphysics, logic, sociology, and the history of philosophy more than in experimental psychology.

The articles I have published have appeared mainly in the *Revue Philosophique,* the *Année Psychologique,* and the *Bulletin de la Société Scientifique et Médicale de l'Ouest.* Most of them have to do with the perceptions of touch, sight, hearing, and the sense of equilibrium. Wishing to have some accurate ideas about the perception of time, I carried on some investigations in this field, the results of which were published in 1907 in the *Revue Philosophique.* One fact had impressed me at the very first: the ease and precision with which we can compare heterogeneous durations, that is, for example, the duration of a sound and that of a light or a pressure. On the contrary, we can compare only roughly heterogeneous intensities or even intensities relating to the same kind of sensations, the intensity of a blue light, for example, and that of a red light. The experiments which I conducted in the perception of durations brought me to the conclusion that, when we can very easily compare the durations of sensations of different senses, it is really because the same kinds of sensations or imagery form the basis of the comparison; thus, for me, the perception of any duration whatever is regularly accompanied by vocal imagery; so, when I think I am comparing the duration of a pressure, for example, and that of a light, what I am really comparing are the durations, of the same nature, of two sounds that I produce mentally.

I have given special study to the problem of the perception of space and have devoted a volume to the visual perception of space. In this volume, I have considered in detail the perception of form, size, position and direction, depth, and movement. I have reported there the principal results obtained by those who have investigated the same perceptions. My personal contribution has been mainly the following: Concerning form, I have tried to find out the accuracy with which we can perceive the straight line, either in direct vision or indirect vision, with eyes fixed, or, on the contrary, with the eye running over it. I have made numerous determinations relating to the perception, either in light or darkness, of position—for example, the median position, that is, the position of an object that we say is exactly opposite us. I have studied experimentally the part played by convergence in the perception of depths and have verified the fact that beyond approximately 15 meters, no difference in the depth of two points can be perceived by means of convergence alone. As to the monocular perception of depth, I have found, as had Hillebrand, that, if the head remains rigid, this perception, when one confronts unknown objects, is very imperfect, two points of light, observed in darkness, appearing at the same distance when one is two meters from the observer and the other about twenty meters (or probably even farther). I have studied also the visual perception of movement and have verified the fact, already affirmed by Aubert, that the speed necessary for perception of movement is much less when the observed object is in the light and surrounded by motionless objects than when, in darkness, it is the only thing visible and one follows its movement with the eyes. I have set forth the hypothesis that the perception of movement (and position) of an isolated object on which the eyes are fixed is due to the tactile sensitivity of the eyelids which, in fact, themselves move, when the direction of our glance changes, rather than to the sensations from the muscles of the eyes.

I have centered my attention upon the study of the illusion which shows us space as limited by a vault, the vault of the sky, and to that of the apparent size of the objects that we perceive on this vault. I have pointed out that the constellations during the night and that, above all, the clouds present the same apparent increase in size as the moon or the sun drawing near the horizon, and my experiments have confirmed for me the classic doctrine to which I firmly adhere, despite the contradictory affirmations of certain observers, that this increase in size has as its essential basis the apparent distance of the objects which is considered greater when they are close to the horizon than when they are far from it. As for the illusion of the heavenly vault, I have brought out the fact that it clearly manifests itself only under conditions in which the stars or numerous little clouds are visible, and that it tends to disappear in monocular vision. Finally, I have assumed that the apparent distance of these objects, stars and clouds, which conditions its form is explained by the same influences which are active in general in the determination of the perception of depth; the greater apparent distance at the horizon than at the zenith is especially caused, I believe, by the fact that between the horizon and ourselves intervene objects of known dimensions, which we have a tendency, consequently, to see with their "real" dimensions, especially with their real dimensions in depth, just as we see a distant man with real dimensions, as tall at 50 meters from us as at 5 meters, because he constitutes a known object and we recognize him.

I have also investigated the spatial perceptions of hearing, touch, and the sense of equilibrium. As for hearing, I have brought out the part played, it seems to me, in certain cases by the more or less great distinctness of the perceptions; that is, the more or less great precision with which are distinguished, for example, the elementary noises which enter into the constitution of a prolonged, complex noise. This rôle of distinctness seems to me to explain the fact that the perception of the distance of an unknown noise, a perception which is, moreover, not very exact, is not better with two ears than with one alone. I have assumed that the lack of distinctness which is evident when a noise draws farther away is caused by the influence exerted by the reflected waves, an influence which would be more marked than when the noise is produced near the ears. The distance of an unknown musical sound, in which the elementary sounds are not distinguished, is perceived very inexactly, which may be explained by its essential lack of distinctness.

As for the spatial perceptions of touch and the sense of equilibrium, I have studied the perception of the verticality of the head and the body, the perception of the movements of our limbs, the perception of the circular movements and the rectilinear movements of the whole body, and the perception of the attitude of the body.

I have tried to show that the tactile perception of the movements of our limbs is not furnished us exclusively either by the sensations of the muscles or by the sensations of the articular surfaces, or by the sensations of the skin. Experimentation on normal man and clinical observations have seemed to me to lead to the conclusion that we perceive these movements principally through the cutaneous or subcutaneous sensations caused by the distention or retraction of the tissues which take part in the movements. I have brought out the following fact, which shows, I believe, the important part played by subcutaneous and cutaneous sensitivity in the perception of movements: if we squeeze with one hand the forearm of another person, a little above the wrist and if we ask this person to make some movements with the hand alone, we ourselves will perceive the movements through the sensations felt in our hand. The non-visual perception of our movements may, moreover, include visual imagery. It may also imply elements furnished by the sense of equilibrium; such is the case when we perceive one of our limbs as moving horizontally or vertically.

As for the position of the head and body, I have tried to find out the precision with which we perceive them and what sensations play a part in these perceptions. I have distinguished between the straight position and the vertical position of the body—the body may be straight and still be inclined in relation to the vertical. With the body upright and free, and the feet resting on a horizontal base, I have found that one can perceive an inclination from this base when it reaches about 0.5°. With the head held still and the body fastened securely by the legs and hips against a movable vertical table, so as to do away with all pressure on the soles of the feet, the lateral inclination that it is necessary to give the body before it ceases to be perceived as in a vertical position may reach 2° or even more. If, with the body held quietly in a vertical position, and the head left free to move, subjects are instructed to place the latter in a vertical position, we find a considerable average variation; again, errors of several degrees may occur when the subject is required to place the head in a vertical position while the body is inclined. These results indicate that it is not the sensations from the head which inform us with precision as to our verticality or our inclination.

Still in the same general field, I have verified, among others, the following facts relating to the perception of the position of the head. When the body is very much inclined, the realization of the erect position of the head, that is, the position which would exist if the head presented no inclination in relation to the body, is very inexact; with the body inclined laterally 20°, the errors often rise above 10°. An exterior force (weight), even a considerable force, which tends to incline the head and which the latter must resist, has no influence upon the apparent verticality of the head. The perception of horizontality of the head is likewise not very exact, even when the body is horizontal.

The results of my experiments upon the passive rectilinear movements of the whole body have led me to conclude that the perception of such movements is not very accurate. An observer, his back against the back of a chair in which he is comfortably seated, still does not recognize without error the direction of a horizontally forward movement when the direction of this movement forms an angle of 20° with the median plane of his head and body and the acceleration reaches about 40 centimeters. Contrary to what Mach found, I have not been able to observe consecutive sensations persisting a long time after the cessation of the movement. The persistence that I have been able to verify was only from a quarter to half a second. The perception is not influenced in any marked degree by the compression of the body; moreover, compression, even when strong, does not succeed in suppressing all slipping of the body on a horizontal table on which it is extended and to which is given an accelerated movement, and this slipping results in distending the skin and may thus arouse cutaneous or sub-cutaneous sensations which are capable of informing us of the movement.

I have made some rather extensive investigations into the passive movements of rotation of the body as a whole. For the most part I have used a horizonal rotating table in these experiments. I have experimented in a sitting position, lying on my back, on my stomach, etc., and giving my head various positions in relation to my body. Among the facts that my experiments have permitted me to verify I will cite the following. The same perceptions may be produced for different positions of the head in relation to the body and in space. If, then, we consider the semicircular canals as organs of sensitivity excited by the rotations, we must also admit that the perceptions felt depend on influences other than those of the sensations immediately caused. The compression of the body has little influence on the perceptions, and a remarkable fact is the distinctness with which the perception of a rotation is distinguished among the painful sensations which a heavy compression may cause. The amplitude of the movement is very inexactly perceived: it may happen at the moment of a rotation, continually felt, that one believes he has made two turns when he has really made only one. I have studied the reflex movements of the body and the eyes provoked by the rotations; I have ascertained by means of after-images that my eyes were turning regularly around axes parallel to the axis of the movement of the body, in a direction contrary to that of the body at the beginning of the rotation, in the direction of the previous movement of the body at the close. This regularity of the movements of the eyes constitutes an objection to the hypothesis sometimes maintained that it is because of these movements that we perceive the rotations. I shall point out also the following interesting illusion which I verified very distinctly. My head being held quiet and the rest of the body submitted to a rotation, it was my head alone that seemed to me to move, unless my attention was concentrated upon my body; in that experiment I was lying on my back on the table and holding between my teeth a thin board which was not attached to the table; the axis of rotation of the table and that of my head coincided, so that my head remained motionless when the table was turning a wee bit; an assistant gave the table, and consequently my body, rapid oscillatory movements between two bumpers.

I have concluded from my experiments in rotation that the head plays an important part in the perception of these rotations and I admit that it is a question, as to this rôle, of excitations of the semicircular canals. But I have also proposed that sensations caused in other regions of the body intervene in the formation of the perceptions considered and that these sensations contribute in making the perceptions exact in certain cases, despite the varied attitudes that the head may have; so, when the body is vertical, the perception of a movement of rotation that it undergoes is almost regularly exact, whatever position one gives the head and, consequently, the semicircular canals. Sitting on the horizontal table, I feel myself always turning like the table, from left to right or from right to left, whether I hold my head erect or, during the whole course of the movement, hold it very much inclined forward. It would no longer be the same thing, as one knows, if, instead of holding the head bent over from the beginning of the movement and holding it quiet, I should wait to incline it until the movement has already lasted a certain time. I shall also add that, even when the head is kept motionless during the whole duration of the movement, the perception may become inexact if the attitude of the body and the head is frankly abnormal: lying on my back, I feel in general, when I perceive the movement, rolling sensations, that is, the rising of the table at my left or at my right; at the sudden stop of the movement of the table, it seemed to me, sometimes for some instants, that I was rolling in one position around the longitudinal axis of my body.

After having at first questioned the existence of a sense of equilibrium, I finally came around to the doctrine defended by Mach, Breuer, and Crum Brown, according to which we have in the utriculus, the sacculus, and the semicircular canals true organs of sensitivity. I think, as I have already pointed out, that the semicircular canals are excited by the movements of rotation (and, in general, by the curvilinear movements) of the head and body and that they contribute in a rather precise fashion to the perception of these movements. The nerve endings of the utriculus and the sacculus undergo the influence of weight and rectilinear movements, but give us less precise information about them than the semicircular canals about curvilinear movements.

I have pointed out, relating to the perception of space in general, an influence which, as far as I know, had never been mentioned by those who have investigated this perception, that of weight. This influence conditions the perceptions of rising and lowering, verticality, horizontality, and the intermediary directions between them. In the curvilinear movements to which the body as a whole may be subjected, when, for example, on the railroad, we go around a curve, to the action of weight is added that of the centrifugal force and, consequently, the objects really vertical or horizontal, seen or touched, cease to appear vertical or horizontal.

I published in 1926 a volume entitled *L'Intelligence;* a better title perhaps would have been *Les Phénomènes Intellectuals;* I proposed, in fact, in this volume to present a clear and elementary analysis of phenomena such as imagery, memories, reasoning, etc., excluding as far as possible the consideration of sensations, feelings, and volitions. Facts, rather than theories, are considered in the work. I show there that there is no essential difference between general ideas and individual ideas, that recognition and meaning are distinct phenomena, that recognition does not consist, as has been claimed, of the evocation or the tendency to evocation of movements habitually associated with the object or the phenomenon recognized. I consider there association as a primitive psychological phenomenon. For the rather metaphysical theory of two kinds of memories admitted by certain philosophers, I substitute the distinction, resting on facts, familiar to all and easy to understand, which is expressed by the words *to be acquainted with* or *to know* and *to remember.* I point out the importance of verbal thought, while noting that it presents nothing specific. I insist somewhat upon verbal associations, which have been the object of numerous investigations and concerning which I myself have conducted systematic experiments at various times. I distinguish three groups of such associations: associations purely verbal, in which the meaning of the word plays no part, grammatical associations, and associations based on the meaning. Each of these groups may be subdivided besides, in conformity with the old distinction of the associative psychologists, into associations by resemblance and associations by contiguity; there is grammatical association through resemblance, for example, when one associates a verb with a verb, an adjective with an adjective; through contiguity when, undergoing the influence of usual syntax, in French one makes an adjective follow a noun, an adverb a verb, etc. (*papier bleu, dormir profondément*). The purely verbal influence, the grammatical influence, and the influence of meaning may, moreover, act simultaneously and cooperate in the evocation of the word that one associates with the word presented.

As I have grown older, I have become less and less interested in theories and hypotheses in psychology. The physiological explanations of psychological phenomena which have sometimes been proposed have too often seemed vague to me, useless as soon as one approaches the details of the phenomena. Two theories, it seems to me, at present hold the attention of psychologists, behaviorism and the theory of form. As for behaviorism, it is really a method rather than a theory, and all the findings that its partisans may make will consequently easily fit in with any psychological system. As for the theory of form, one may distinguish between its application to the physiological phenomena which are produced in the nervous centers and its explanation of psychological phenomena. In the case of physiological phenomena, the conception of a dynamism that it opposes to mechanism lacks precision and has up to the present led to the discovery of no new fact. In the case of psychological phenomena, the theory has suggested a rather considerable number of experimental investigations which have incontestably given interesting results. It has brought out especially the unity of complex psychological phenomena such as perceptions, the primitive character of this unity, the falsity of certain theories of association, the fact that in our perceptions the parts that one may artificially distinguish are not often in reality distinguished and, consequently, will be useless to provoke the imagery of the wholes to which they belong; it has emphatically called attention to the organization which is found in perceptions and which has as its effect that the constituent parts of these perceptions act upon one another, each contributing to determine the others and being determined by them, and, consequently, being able to be different in a given whole from what it is in another whole. But we may note that many of the facts brought out by the partisans of the theory of form had been already set forth more or less clearly before the development of this theory.

A theory about which I have made up my mind is dualism. This theory, since Descartes, has not ceased to have numerous representatives among the philosophers and psychologists. The theories of parallelism, of interaction, assume it. The radical opposition of the sensation and the exciting force that we note in psychophysicists like Fechner assumes it equally. I think that this theory is false, that psychological, physical, physiological phenomena should not be opposed to one another, that, as Mach declared, it is a mere question, when one opposes, for example, psychology and physics, of a difference in the orientation of our interest and attention. "The world consists of colors, sounds, warmths, pressures, spaces, times, etc. . . . As long as, neglecting our own body, we occupy ourselves with the mutual dependence between these groups of elements which constitute foreign bodies, including men and animals, we remain physicists. We study, for example, the modification of the red color of a body caused by a modification of the brightness. When, on the contrary, we consider the special influence on the red of these elements which constitute our body, . . . we are in the field of physiological psychology."[2]

INFERENCES

To this question which is asked: What will probably be the development of psychology during the next generation? I hesitate to reply, knowing how imprudent it is to wish to prophesy. It seems to me, however, that we may foresee a more or less precise analysis of sensations and perceptions, facilitated, because of the progress of physics and chemistry and their methods, by the use of improved techniques.

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[2] Mach, Populär-wissenschaftliche Vorlesungen (2 Aufl.), p. 231.

The study of sensations and perceptions has reached at present a high degree of exactitude. Hence we may assume that the psychologists will attack more and more one of those less known phenomena, the phenomena of thought, feelings, volitions, and actions.

Abnormal psychology will doubtless continue to furnish numerous bits of information that may be used in normal psychology, which, in its turn, by its analyses, will guide the observations of the pathologists. Works such as those of Head show how normal psychology and abnormal psychology may progress simultaneously through association.

Psychological phenomena depend upon physical and physiological conditions. Both are only incompletely known. The physiological conditions in particular are still almost completely unknown. We shall doubtless try to substitute positive facts for the vague hypotheses to which we are at present reduced as regards these conditions. The impossibility of experimenting as we should like on man unfortunately makes the investigation of these conditions very difficult.

At the same time as the development of general psychology, we witness that of special psychological studies. Abnormal psychology, already old, continues and will continue, without doubt, to progress. At the same time, we see the development of the psychology of the primitive, that of children, that of animals.

It is to be desired that individual psychology, because of its usefulness, will be the object of more and more numerous investigations. A problem inadequately solved and which always presents great interest is that of the influences which determine the development of the individual. Do innate, inherited abilities exist and to what extent? What is the influence of environment, of education?

JAMES DREVER

Introduction—Early Years

I was born in the village of Balfour in the island of Shapinsay, one of the Orkney Islands, on the 8th of April, 1873. The Drever family seems to have belonged originally to the island of Westray, mention being made of Drevers in the records of that island as early as the sixteenth century. Our branch of the family, however, had been settled in Shapinsay for several generations, and for at least two generations the landlord of the island had drawn from the family his builders and masons. My grandfather, accordingly, was the landlord's principal builder, and his three sons followed in his footsteps. My father, the second son, more restless or more ambitious than his brothers, had the presumption, some four years after my birth—I was the oldest of a family of four—to undertake the work of building a new pier in the town of Stromness, on the Mainland of the Orkneys. The landlord was highly incensed and evicted our family from their small cottage in Balfour. We therefore migrated to Stromness, and from that point my own continuous recollections begin. Except for a year in the island of Stronsay, when a pier was being constructed there, the remainder of my boyhood up to the time of leaving for the university was spent in Stromness.

As a boy in Stromness, my life was pretty much that of the other boys in the town. Educated at the public school, playing the various games that were played, bathing and learning to swim in the summer, skating and 'sledging'—as we called tobogganing—in the winter, sailing a boat at most times during the year, except for the stormiest months of the winter, I have nothing particular to chronicle except for the fact that from being a rather delicate and sickly child I grew up into a hardy and athletic youth, with some skill in outdoor sports and in the handling of a boat under sail, the latter of which has remained my favorite recreation.

Conditions in Stromness at that time must have been fairly favorable for intellectual development, since I found myself at thirteen reading with pleasure and understanding Thackeray, Ruskin, Carlyle, and Emerson. For several years these were my favorite authors, and Thackeray remains a prime favorite to this day. Gifted with the capacity of learning by heart rapidly and accurately, I could repeat pages of these authors. I have very clear recollections of one of my uncles entertaining himself with this capacity by getting me to perform for him various feats of memory. He would read eight lines of verse to me and ask me to repeat after hearing once, a feat I could usually perform successfully. My retention was also good. At one time I could repeat the whole of the first book of *Paradise Lost,* and even yet I can readily recall some hundreds of lines of it. This memory was naturally of great assistance in school work, in which I was not undistinguished. In 1886 I took first place for Orkney and Shetland in the Orkney and Zetland Association's annual examination, and in the following year was equal first for Scotland in the St. Andrews University Local Examination.

These school successes practically decided my future. I was to be a teacher. At that time the pupil-teacher system still existed in Scotland. Accordingly, I was indentured for four years as a pupil-teacher with the munificent salary of £40 for the four years. I was then fourteen. Ordinarily the pupil-teacher had to take charge of a class, his own education being continued in the early morning before regular school began. As it happened, however, the headmaster, who himself taught the upper two classes, required assistance, and I was therefore selected for this work. Though teaching various classes in Latin and mathematics, I did not have full responsibility for any class. This went on for two years. Realizing then that, except for the fact that I was in daily contact with one of the finest teachers I have ever met, and doubtless gaining much from his example, and in experience of teaching, I was really wasting my time as far as my own education was concerned, I broke my indenture, paid the penalty attached, and went to the university.

I. University Career

In October, 1889, I matriculated for the first time as a student of the University of Edinburgh. I was then sixteen, but probably as mature in outlook as most of the youths now entering the University at eighteen. At least, that is my impression. The curriculum at that time for the degree of Master of Arts in the Scottish universities was the old seven-subject curriculum in Latin, Greek, mathematics, natural philosophy, logic and psychology (metaphysics), moral philosophy, rhetoric and English literature. All candidates for the degree had to profess these subjects, and to pass three examinations, the first in Latin and Greek, the second in mathematics and natural philosophy, and the third in philosophy and English. By passing a special preliminary examination in Latin, Greek, and mathematics, a student obtained the privilege of completing the course in three years. Otherwise it occupied four years. Most students of my age, and practically all students who had not attended one of the larger secondary schools, took four years. In the first year were taken the Junior Classes in Latin and Greek, in the second the Senior Classes in these subjects with the Junior Class in Mathematics, in the third the Senior Class in Mathematics, the Class in Natural Philosophy, and the Class in Logic and Psychology (or Metaphysics), and in the fourth the Class in Moral Philosophy and the Class in Rhetoric and English Literature. This was the usual order, but I transposed my third and fourth years, or at least left Senior Mathematics and Natural Philosophy for my fourth year, taking the two Philosophy Classes and English in my third.

My professors were: Sellar, and afterwards Burnet, the Aristotelian scholar, for Latin, Butcher for Greek, Chrystal for mathematics, Tait for natural philosophy, Andrew Seth for logic and psychology (metaphysics), Calderwood for moral philosophy, and Masson for English. I doubt whether the Arts Faculty in Edinburgh could muster a group of men of such calibre at the present time. The only one of them now surviving[1] is Andrew Seth Pringle-Pattison, who first introduced me to psychology, and who has now been retired from teaching, though still actively engaged in writing, for many years.

In spite of the fact that I had come from a school in a remote district, I found that my attainments in Latin and Greek were by no means inferior to those of the average student. My Greek was perhaps a little shaky, but not to such an extent as to prevent my thorough enjoyment of the class work, or to keep me from joining a small optional class in Greek verse composition. I read widely, if not very thoroughly, in Greek literature, and took special delight in Butcher's lectures on Greek tragedy during my first year, and on Aristotle's theory of poetry in my second. In all probability my first interest in psychological problems was due to these lectures, though I did not at the time know that they were psychological problems. In particular, Aristotle's theory of *καθάρσἰς* fascinated me. My lasting interest in the affective and emotional aspects of the mental life is possibly to be dated from these lectures.

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[1] Died September, 1931, just after this was written.

The third year at the University really decided my future career, but it was not until after many years that I realized the fact. In that year I attended the Classes of Logic and Psychology (Metaphysics), Moral Philosophy, and Rhetoric and English Literature, as I have already indicated. It was Andrew Seth's first year as Professor of Logic and Metaphysics at Edinburgh, Campbell Fraser having just retired. To Andrew Seth I owe my first introduction to psychology as a systematic study. I am afraid he made us see James, Sully, and Wundt through his spectacles, but all the same he gave us a very sound understanding of the basal principles of introspective psychology—the psychology of Ward and Stout. He was no mere partisan in his teaching or in his outlook. With the new experimental psychology, then developing rapidly under the influence of Wundt in Germany, he himself had not a great deal of sympathy, but he did not leave his students in ignorance, as many professors holding his opinions would have done, of the fact that such a development was taking place. There was no prescribed textbook for psychology, but James's *Principles* had just been published, and, as one might expect, was frequently referred to, while the general point of view of the lectures was that of Ward's *Encyclopaedia Britannica* article.

Henry Calderwood was Professor of Moral Philosophy. Taking the class out of its proper sequence, I found myself considerably handicapped at the start. The work of the Class of Logic and Psychology was assumed. Consequently, many parts of the earlier lectures were for me scarcely intelligible. It was indeed only towards the end of the session that I found my feet. The Professor's *Handbook of Moral Philosophy* was the textbook. Even this, lucid as it is in style, and relatively simple in content, presented considerable difficulties, owing to the fact that the significance of the various points was not fully comprehended until half through the session. Calderwood was himself a very fine lecturer, and it was no fault of his that I found myself floundering in the mire for some months before I gained solid ground.

David Masson, I have said, was Professor of Rhetoric and English Literature. No one who ever sat under Masson could fail to be influenced by him, and this was the class to which I proposed at first to devote my chief attention. The attractions of psychology, however, as presented by Andrew Seth, diverted me from this intention, and halfway through the first term I found myself neglecting my reading for the English Class, in order to read Sully, and James and Ladd, and—strangely enough—Herbert Spencer. The net result of the year's work—my best year at the University so far—was a medal in logic and psychology, a prize in English, a second-class certificate of distinction in moral philosophy, and a permanent interest in psychological problems.

The fourth year at the University was something of an interlude in my psychological and philosophical studies. Mathematics under Chrystal and physics under Tait had still to be taken for my Master's Degree. Mathematics did not worry me. That had been my best subject at school, and I probably knew enough mathematics when I came up to the University to see me safely through the degree examination. With physics it was different. No physics had been done at school, and the subject, especially the dynamics part, presented unexpected difficulties. Nevertheless, the examination at the end of the year was successfully passed, and in April, 1893, I graduated Master of Arts at Edinburgh, having just passed my twentieth birthday.

The future at this time looked rather dark. I wished to study medicine. Financial resources at home were strained, and it was by no means clear how the expense could be met. A temporary teaching post during the summer solved the immediate problem, and the following session saw me back at the University as a registered student of medicine, but taking also the Advanced Class in Logic and Metaphysics under Seth. A temporary teaching post during the summer once more enabled me to put in yet another year studying medicine and philosophy. Then came temporary defeat. As the result of a letter from Seth, Schurmann of Cornell wrote me inviting me to apply for a scholarship at Cornell, but my eyesight was giving me trouble, and I was on the verge of a physical breakdown. It was obvious that a long rest was absolutely essential. Financial conditions at home were such that a complete rest was impossible, and it seemed clear that any thought of studying philosophy at Cornell, or of going on with the study of medicine at Edinburgh, must be definitely abandoned. Wavering for some time between the Church and the teaching profession, I ultimately decided to enter the latter. The next eleven years, except for the winter session 1897-1898, were spent in teaching in various schools, and in various capacities. Much valuable educational experience was gained in these years, and a great deal of miscellaneous reading done, all of which bore fruit later.

The winter session 1897-1898 was again spent at the University. Two classes were attended—Theory and History of Education under Simon Laurie, and Political Economy under Shield Nicholson. Both left a deep and lasting impression. Laurie was the foremost educationist of his time in the English-speaking countries. He was also no mean philosopher and psychologist, and it was mainly to attend his lectures that I had returned to the University. Laurie established finally that interest in psychology which had been stimulated by Andrew Seth. His philosophy and psychology were those of the Scottish School, the influence of German philosophy being relatively slight as compared with its influence on Seth's teaching. From his philosophy and psychology Laurie deduced his educational theory. He built up in the minds of his students a marvellously complete and logical philosophy of education, which in its turn supported an educational psychology and an educational theory, coherent and closely articulated throughout. No weak points were discernible in the lecture room, though several became apparent later in actual dealing with concrete children in the school. The mark which Laurie—and Darroch, who followed him in the Chair of Education—left on Scottish education, and particularly on the training of the teacher in Scotland is profound.

Hitherto I had done comparatively little science in any real sense. It is true I had attended Tait's class in physics, and I had made a beginning with medical studies. The scientific point of view, however, had never been brought home to me, and experimental science had been a routine rather than a quest for truth. The influences which turned me to serious scientific studies I am now unable to trace. About 1900 I had begun to make definite efforts to arrange my reading and my vacation studies with reference to a London Science Degree. Little could be done for the next three years, during which I was Headmaster of the Central School in the Island of Stronsay, Orkneys. Coming to the conclusion that I was in danger of finding myself in a *cul de sac,* I resigned the headmastership, and returned to Edinburgh, obtaining a position as Assistant Teacher, first in Flora Stevenson School, under the Edinburgh School Board, and after a year there in George Watson's Boys' College.

All the spare time of the next few years was devoted to science studies—geology and physiology at the University, physics and chemistry at Heriot-Watt College. The Intermediate Science Examination of the University of London was passed in 1905 in mathematics, physics, chemistry, and geology. In the following year, when on the point of entering for the Final Examination in physics, chemistry, and psychology, I was invited by Professor Darroch, who had then succeeded Laurie as Professor of Education at Edinburgh, to become his Assistant at the University. The first result of this was to cause my science studies, except in psychology, to be suspended at least temporarily, and ultimately to make an Honors Degree in Psychology the aim, in place of a general Science Degree.

II. The Coming of Experimental Psychology

Very important changes were at this time taking place in the training of Scottish teachers. Hitherto the Training Colleges for Teachers had been institutions established and—nominally—controlled by the Church. In 1905 the Scottish Education Department brought into existence four Provincial Committees for the Training of Teachers, one in each of the four university centers, Edinburgh, Glasgow, St. Andrews (Dundee), and Aberdeen. These Committees were constituted by representatives of the School Boards, the secondary schools, the teachers, and the University in each area, and were charged with the professional education and training of all grades of teachers. Each Committee took over the Training Colleges in its area, where such existed, and proceeded to reorganize the whole system of training in accordance with the new regulations which came into force in 1906.

These changes brought in their train many important developments. It was hoped that the universities would take a larger share in the education and training of teachers, and increasingly so as time went on. As a step in this direction university lecturers were placed in charge of several of the Training College courses. Moreover, the new regulations for the training of teachers required that all teachers in training should be given a course in psychology. This led not only to the establishing of psychology departments, but also to the institution of such departments in the universities themselves. In Aberdeen there had long been a lectureship in comparative psychology, but in all the other universities psychology had been taught as part of the course in logic and metaphysics. Apart from the existing lectureship in Aberdeen, the first of the universities to establish an independent lectureship in psychology was Edinburgh.

George Combe, best known now as a phrenologist, but with a still better claim to remembrance as an educationist, had bequeathed his estate in trust for the "education of the poorer classes in the laws of mental and bodily health." The annual income from the estate had up to this time been applied by the George Combe Trustees to the payment of itinerant lecturers, who delivered popular lectures, mainly on physiology, in different parts of the country. At this time, the matter of the teaching of psychology in the universities being in the air, Professor Sir Edward Sharpey Schafer, occupying the Chair of Physiology at Edinburgh, and Professor Andrew Seth Pringle-Pattison, occupying the Chair of Logic and Metaphysics, approached the Trustees, with a view to securing their assistance in establishing a lectureship in psychology in the University of Edinburgh. The Trustees agreed to give an annual grant over a period of five years, amounting in the first instance to five-eighths of the proposed salary of the lecturer, and also to make a grant towards the equipment of a psychological laboratory. Certain conditions were laid down: (1) that the lectureship should be called the George Combe Lectureship in General and Experimental Psychology, and that it should be independent of any existing chair or lectureship, (2) that the George Combe Lecturer should make provision for the teaching of applied psychology, particularly as regards the application of psychology to education, (3) that the Lecturer should be appointed by the University Court and the George Combe Trustees jointly, and (4) that the Lecturer should report annually to the Trustees with reference to the success of the Lectureship.

In this way the George Combe Department of Psychology came into existence. The annual subvention from the Trustees has been renewed at five-year intervals up to the present time, and has been several times increased as the work of the Department expanded, while the whole cost of the equipment of the laboratory has been borne by the Trustees. In 1906 Dr. W. G. Smith was appointed as first Combe Lecturer. During the first year of his tenure of the Lectureship he delivered a short course of lectures on general psychology as part of the course in logic and metaphysics for the M.A. Degree, and conducted a course in experimental psychology in the Physiological Laboratory, pending the reconstruction of part of the space vacated by the Physics Department as a psychological laboratory. In the following year a full graduating course in the Faculty of Arts was offered in general and experimental psychology, and Dr. Smith entered into occupation of his new laboratory.

In no other Scottish university was psychology given the status of a full graduating course in Arts, on an equal footing with logic, moral philosophy, and the older Arts subjects. In Glasgow, where a lectureship in psychology was also instituted, and placed under the charge of Dr. H. G. Watt, and in Aberdeen, psychology could only be taken as a second course after logic. St. Andrews, which had the good fortune to possess Stout as Professor of Philosophy, did not establish a lectureship in psychology until much later. The establishment at this time of a psychological laboratory in Edinburgh meant a great deal to me in the prosecution of my psychological studies. Hitherto, practical laboratory experience in psychology had not been easy to obtain. By the kindness of Professor Sir Edward Schafer and Dr. Sutherland Simpson I had been permitted to carry out in the Physiological Laboratory at Edinburgh such psychological experiments as the resources of that laboratory made possible. Similarly, by the kindness of Dr. C. S. Myers and Dr. William Brown a similar privilege was accorded me in the Psychological Laboratory of King's College, London. I could only avail myself of this latter privilege during summer vacations, and in both cases great difficulty was experienced in obtaining subjects. When the Psychological Laboratory in Edinburgh was opened, Dr. Smith at once placed its resources at my disposal for experimental work, but the same difficulty of finding subjects was still experienced. The manner in which this difficulty was surmounted is interesting, though probably by no means unusual in the history of experimental psychology. I married in December, 1907, and thereafter my wife was always available as a willing subject. It thus became at last possible to work through the experiments in Titchener's *Text-Book of Experimental Psychology* in something like a systematic manner.

For some time I had contemplated taking the B.Sc. of the University of London by research in psychology. Ultimately, however, I decided to take it by examination in the usual way, though from 1907 onwards I was recognized as a research student of the University of London, working on the subject of fatigue. Some of the results of this research work were subsequently published in *Child Study* in an article entitled "A New Test for Fatigue." In 1909 I sat the final examination for the London degree, and graduated as

B.Sc. with first-class honors in psychology, my subsidiary subject being physics. In the following year I became a research fellow in psychology of the University of Edinburgh, and began the work on instinct which led ultimately to the conferring on me of the degree of D.Phil. This degree has now been discontinued by Scottish universities owing to the risk of confusion with the new Ph.D. degree. It was conferred only on *honors* graduates after at least five years of research work. My D.Phil. was conferred in 1916, the thesis for the degree being *Instinct in Man: a Contribution to the Psychology of Education,* published under that title by the Cambridge University Press in 1917.

III. LECTURER IN EDUCATION

While prosecuting my studies in psychology, first for the B.Sc. of London, and then for the D.Phil. of Edinburgh, I was at the same time, from 1907 onwards, acting as Assistant to the Professor of Education in Edinburgh, and lecturing on the theory, history, and psychology of education to university students in training as teachers under the Edinburgh Provincial Committee for the Training of Teachers.

Armed with a letter of introduction from the Scottish Education Department, I spent some months of 1908 and 1909 visiting the secondary schools of Germany, and studying the German methods of training both primary and secondary teachers. The chief reason for these visits to Germany was the institution at the University of a new course in present-day educational systems and problems, of which I had been put in charge, and one result was the publication of several papers on German education in *School.* Another visit was paid to Germany in the summer of 1913, and some weeks spent in Meumann's laboratory in Hamburg. The object of this third visit was to study the equipment of the laboratory, more particularly from the point of view of experimental pedagogy. It had been found possible to develop at Edinburgh Training Center a course in experimental pedagogy—now experimental education—for advanced students. When the new college at Moray House was built in 1912, accommodation was provided for a pedagogical laboratory, and, as responsible for the course in experimental pedagogy, I was also responsible for the equipment of the laboratory. In effect this was a laboratory for educational psychology, and it was the first such laboratory in the British Isles. While its chief function was the introduction of the more advanced students to the application of experimental methods to educational problems, it was also intended to offer facilities for original investigation. Some work on fatigue in schools, on children's vocabularies, and on the analytical study of reading and writing was done, and a number of papers appeared in various journals during the next six years. The most important of these were: "A Study of Children's Vocabularies," "The Vocabulary of a Free Kindergarten Child," "Notes on the Experimental Study of Writing," and "The Analytical Study of the Mechanism of Writing."

As has been indicated, this work in educational psychology represented only one side of my activities during these years. I was also lecturing on the theory and history of education, and my published work included a book on *Greek Education: Its Principles and Practice,* published by the Cambridge University Press in 1912, the chapters on Greek and Roman education in *The Teachers' Encyclopaedia,* and several papers on educational subjects, the most important of which were two on the "Training of Teachers" in the *Journal of Education.* A movement was at this time on foot to get a postgraduate degree in education established in Edinburgh. Dr. Alexander Morgan, Principal of the Training College, at Moray House, was a strong advocate of such a degree. He argued rightly that a Teachers' College, more or less analogous to Teachers College, Columbia University, could easily be developed in Edinburgh, the relations between University and College being what they were. Without going quite so far as he wished to go, I was convinced of the general desirability and practicability of instituting a postgraduate degree in education, and joined with him in urging my chief, Professor Darroch, to induce the University to take the necessary steps.

For some years Professor Darroch hesitated. The institution of a new degree in a Scottish university requires an Ordinance, which is virtually an Act of Parliament. To the passing of an Ordinance the other Scottish universities may object, and a complex and difficult situation may arise unless the universities are in general agreement. On the other hand, a Scottish university is quite autonomous as regards the institution of a diploma. At first Professor Darroch was inclined to think that a diploma in education would satisfy all needs. Accordingly, he prevailed on the university authorities in Edinburgh to institute a post-graduate diploma in education in 1914.

Candidates for this diploma were required to take their M.A., and complete their practical training with credit, and thereafter to attend the University for an additional year, passing at the end of the year an examination in psychology and in education on a higher standard than that required for the M.A.

This was really a very important development from the point of view of the teaching of psychology in Scottish universities. It gave psychology a definite place as a compulsory subject in a diploma course. The diploma in education attracted students immediately. Several of the students were prepared to carry on still further their studies in psychology and education, so that Professor Darroch and the University authorities were at last convinced that a degree in education was desirable. Accordingly, an Ordinance was prepared instituting such a degree, and, after various unforeseen delays, this Ordinance came into force in 1917. The Universities of Glasgow and Aberdeen promoted similar Ordinances, the Glasgow Ordinance also coming into force in 1917, and the Aberdeen Ordinance a year later. For the degree of Bachelor of Education at Edinburgh (B.Ed.) the student was required to spend two years in post-graduate study—subsequent, that is, to his course for the M.A. or B.Sc.—the subjects studied in these years being psychology and education. He was also required to pass an examination in these subjects at the end of each year. The examination at the end of the first year was the already existing diploma examination. The final year was thus devoted to the advanced study of psychology, educational psychology (experimental education), and education. The result, as far as psychology was concerned, was the establishment in Edinburgh of a full two years' course in psychology, and the opening up of possibilities for research work by giving the students the necessary preparation for psychological research.

IV. The Development of a Point of View

While psychology was becoming strongly established in the University in the manner described, I was engaged, on the one hand, in lecturing on education in the University and at Moray House, and on psychology and educational psychology (experimental education) at Moray House, and, on the other hand, in the investigation of the problems of instinct in connection with my D.Phil. thesis. The original stimulus to the work on instinct was given by McDougall's *Social Psychology,* which first appeared in 1908. Approaching it at first from the point of view of a student of education, I was disposed, like a great many British educationists, to accept the author's theories almost in their entirety. The attention of British educationists at the moment was largely concentrated on problems of moral education, and there was somewhat acute controversy between the advocates of direct and of indirect moral instruction. The Edinburgh school of education, under the leadership of Darroch, was strongly opposed to direct moral instruction. The teaching of McDougall could be fitted very nicely into a logical and well-articulated theory of moral education as presented in Edinburgh. Accordingly, McDougall's *Social Psychology* was at once made one of the textbooks for the university class in education.

This rather uncritical acceptance on my part of McDougall's theories was brought to an end by the symposium on "Instinct and Intelligence" of 1909.[2] It was this symposium that sent me to the investigation of the history of the concepts of "instinct." As regards the question at issue in the symposium, I was at first strongly attracted to the views presented by Myers, but, as my investigation proceeded, I found my views approximating more and more closely to those of McDougall. The development of my thought at this time can be partly traced in the chapter on "Instinct and Intelligence" in my *Instinct in Man.* I began to write this chapter with the feeling that my views more or less coincided with those of Myers; I finished it in essential agreement with McDougall on the main points raised. McDougall's account of the relation between instinct and emotion, however, I found myself more and more disinclined to accept. In the first place he seemed to be in error in taking emotion as a definite entity—as a mental element, in fact. In the second place, he also seemed to be in error in maintaining that emotions differed qualitatively from one another *qua* affective, and thus represented ultimate feeling elements. In the third place, he seemed to have insufficient grounds for maintaining that an instinctive response is necessarily emotional. From the beginning, I felt convinced that the only sound view of emotion is to regard it as a complex state of the whole organism, and, on the mental side, as a complete "psychosis" involving all three ultimate aspects of the mental life. Emotion being regarded in this way, the qualitative differences between different emotions are not necessarily dependent on ultimate and elementary feeling differences, but may quite well depend on cognitive or conative factors, or both.

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[2] *Brit. J. Psychol.,* 1909, **3**.

With reference to the third point, my views developed gradually. The first stage of this development is represented by the predominantly destructive criticism directed against McDougall in the text of my *Instinct in Man*. The second phase is represented by the appendix to the second edition, which was originally read as a paper before a general meeting of the British Psychological Society in London. This may be said to exhibit the first attempt at a constructive theory. The third phase appears in the brief discussion of the relation of instinct and emotion in my *Introduction to the Psychology of Education* published in 1923. The conclusions at which I finally arrived were briefly these:

1. Emotion is best regarded as a phase of instinctive response.
2. This phase supervenes either when the instinctive impulse is obstructed, or when it is unexpectedly facilitated.
3. On the affective side, emotion shows, correspondingly, that bipolarity which is universally characteristic of our affective experience, pleasure and unpleasure in simple feeling being represented by joy and sorrow in emotion.

Another divergence from McDougall's teaching is my recognition of the two types of instinctive impulse and the two types of interest, which I have called "appetitive" and "reactive," respectively. This distinction is as old as Plato, and seems to me both sound and worth drawing, at least with reference to human psychology. Apart from these divergences of opinion, and even in some of them, my views are in the main the logical outcome of McDougall's own original position, more so perhaps than his own later views as found in his *Outline*. In particular, my doctrine of "joy" and "sorrow" emotions is founded on McDougall's discussion of joy in his *Social Psychology*. Moreover, I should be prepared to subscribe to the earlier "purposivism" of McDougall, which seems to me a sane "behaviorism," but would not follow him in his later "purposivism." Other writers who have influenced my views regarding emotion are Ribot, Rivers, and M. Larguier des Bancels.

From Lectureship to Chair

The lamented death of Dr. W. G. Smith in the influenza epidemic in the autumn of 1918 left the Psychology Department without any staff, since Dr. Smith was at that time running the Department alone. Owing to the War, the classes were not large, and consisted almost entirely of women, but it was nevertheless necessary to provide for the completion of the courses going on. Accordingly, Mr. Henry Barker took charge of the lecture courses, and I took charge of the experimental work. In May of the following year I was transferred to the Psychology Department as Combe Lecturer in charge of the Department, and resigned my posts as lecturer in education in the University and at Moray House.

I cannot forbear at this point paying a tribute to Dr. Smith's invaluable services to psychology in Edinburgh and, indeed, in Scotland. In spite of the handicap of relatively indifferent physical health, he had in twelve years built up and organized, almost without assistance, a large and important University Department. The laboratory under his care had been equipped in a manner and on a scale second to no psychological laboratory in the British Isles. His successor alone can appreciate the magnitude of the work he had done. There can be no doubt that he practically gave his life to the Psychology Department in Edinburgh, and the subsequent development of the Edinburgh school owes almost everything to the pioneer work done by him.

The remainder of my history must be the history of that development, since my personal history has been bound up with the history of the Psychology Department. Since 1919 there has been continued and almost unprecedented expansion. In that year there were eighty students in all in the Department; in 1930-1931 there were five hundred and forty-one. In 1919-1920 two courses only were given; in 1930-1931 there were ten courses. In 1919-1920 the staff consisted of a lecturer and an assistant—Mary Collins, one of Dr. Smith's pupils; in 1930-1931 there were on the staff a reader, two lecturers, two assistants, and four demonstrators or instructors.

The chief events occurring during this period may be briefly chronicled. In 1921 the British Association for the Advancement of Science held its annual meeting in Edinburgh, and incidentally gave a great stimulus to psychology in Edinburgh. It was the first occasion on which there had been a separate section for psychology (Section J.). Previously psychology had been a sub-section under physiology. Lloyd Morgan was the first president of the new section, Cyril Burt was the recorder. Rivers, Myers, Pear, and most of the British psychologists were present, and Langfeld was a welcome visitor from the other side of the Atlantic. The citizens of Edinburgh responded fittingly to the occasion, and gave the new section a good send-off.

Another event which stimulated psychology in Edinburgh was the visit of Morton Prince in 1924. I had met him the previous year at the International Congress in Oxford, and arranged with him then that he should visit Edinburgh the following year, and deliver there a course of six lectures on the "Psychology of the Unconscious." These lectures he delivered in the first instance in Cambridge. His welcome in Edinburgh was of the warmest description. The psychology lecture theatre was crowded to its fullest capacity at every lecture, the press published full reports, and the greatest enthusiasm prevailed throughout. These lectures did much to develop a closer understanding between psychology and the Faculty of Medicine. For some years there had been an optional course in psychology for medical students, as well as a course in experimental psychology for candidates for the post-graduate diploma in psychiatry. The optional course had never been an unqualified success, chiefly owing to the crowded character of the medical curriculum, and the virtual impossibility experienced by the medical student of getting a free period in which to attend an optional course. Ultimately, the professor of psychiatry, Professor George Robertson, who was strongly in favor of bringing psychology into the curriculum of the medical student, succeeded in inducing the Faculty of Medicine to make a short course in psychology compulsory as part of the course in psychiatry.

The year 1924 also saw an advance in the university status of the subject. Previously the Combe Lecturer, though the Head of the Psychology Department, ranked only as a University Lecturer, appointed on a five-year basis, and with a seat on neither Faculty nor Senatus. In 1924, however, the Combe Lecturer became a Reader, with life tenure and a seat on both Faculty and Senatus. The next step, and a necessary step in the opinion of the University authorities, was the establishment of a Chair in Psychology. There were, however, certain difficulties in the way. A professorial Chair in a Scottish university is established by Ordinance, which, as we have already seen, is of the nature of an Act of Parliament. Since a Chair is presumed to be established in perpetuity, it is natural that certain precautions are taken before a university saddles itself with the responsibility of a new Chair, the most important of these precautions being the securing of an endowment sufficient to provide at least a fair proportion of the Professor's salary. The minimum endowment for this purpose at Edinburgh is fixed at £15,000. Until such a sum was forthcoming, it was out of the question to proceed with the Chair in Psychology. In 1930, however, the Carnegie Trust for the Universities of Scotland approved of the setting apart of £15,000 from their quinquennial grant to the University of Edinburgh. Accordingly, the Ordinance establishing a Chair in Psychology was proceeded with, and received the signature of His Majesty on the 19th of May, 1931. At the next meeting of the University Court I was appointed the first Professor of Psychology in the University of Edinburgh. The British universities have been slow to establish professorships of psychology. Previous to 1931 there were only two such in the British Isles—Manchester and University College, London. Cambridge and Edinburgh made in 1931 the third and the fourth. From the point of view, therefore, of its bearing on the academic status of psychology in Britain, the establishing of a Chair of Psychology at Edinburgh must be regarded as an event of the first importance.

The only other event calling for particular notice has been the development of the University Psychological Clinic for Children and Juveniles. While serving in the Education Department, I had become keenly interested in problems of juvenile delinquency. It seemed to me that this was a direction in which the psychologist was peculiarly fitted to perform a social service of great value. In 1925 work was being done in the laboratory—chiefly by Dr. Collins—on character and temperament tests, and this work suggested the possibility of making a systematic attempt to study delinquent cases clinically. Accordingly, clinical work with delinquents was begun on a small scale in that year. At first, cases were seen twice a week—on Tuesday and Thursday afternoons—but later it was found that a more satisfactory arrangement was to give attendance in the Department for new cases on Saturday forenoons, and then to make subsequent appointments for any day that might be most convenient. My interest at this time is represented by my Presidential Address to the Psychology Section at the Oxford Meeting of the British Association in 1926 on "Psychological Aspects of Our Penal System." The workers in the Clinic were all voluntary workers. At first, relatively few cases presented themselves, but as time went on the number of cases increased, until as many cases as could be dealt with were being seen. The type of cases brought also changed very considerably. At first, only delinquents—for the most part court cases—were brought, but as time went on behavior problems of all kinds and educational problems began to be more and more frequently brought for advice. In 1931, at the instigation of Professor George Robertson, the Board of Managers of the Royal Edinburgh Hospital for Mental Disorders placed at the disposal of the Clinic a very suitable house at 37 Morningside Park. To these premises the Clinic removed in February, 1931, and the work was organized on a much wider scale. As before, voluntary service was given by the Psychology Staff, but this was now sufficiently large to enable the Clinic to be kept open every afternoon except Saturday and on Saturday forenoon. Close relations have also been established with Jordanburn Nerve Hospital, the Royal Sick Children's Hospital, and the Royal Infirmary, so that in this field, also, psychology and medicine have come into more intimate contact with one another. The rapprochement between psychology and medicine might indeed be said to characterize to a marked degree the situation in Edinburgh in recent years.

Little remains to be added. The experimental output from the laboratory since I took charge in 1918 has been along three main lines: color vision, on which Dr. Collins has been working for several years; delinquency; and the psychology of the deaf. Work in all three directions is still continuing.

KNIGHT DUNLAP

In August of 1895 I entered the University of California with a maximal outfit of failures and conditions in entrance subjects. Why I was let in at all is still a puzzle to me, as my preparation was mostly lacking. I had attended the usual district school, in which thirty or more pupils were instructed in reading, writing, arithmetic, grammar, geography, history (and sometimes bookkeeping), by one teacher. There was, however, one study by which I profited immensely: "Word Analysis," using Swinton's text. I suspect that from this book I received an impetus towards exact usage of words and precise thinking, and the roots of an impatience (which still grows) with messy thinking which proceeds from the nebulous use of terms. Fortunately, the school year was seldom more than six months in length. Having exhausted the local possibilities, I had attended a newly organized "high school" in the county seat, riding a horse daily from the ranch. This alleged high school was presided over by an amiable ex–Wells-Fargo express messenger, who, finding his travelling occupation was ruining his kidneys, took to the easier occupation of teaching. There were about forty of us in the single room of this "high school," in which the Principal was the whole of the teaching staff. What we of the upper age group studied *in toto* was negligible, except for algebra and plane geometry. Our instructor knew no more of these subjects than we did, but we were interested in them, and dug something out of them.

To enter the university, I took entrance examinations in most of the subjects required for admission to the only division it was possible for me to squeeze into (social science): I flunked several, including algebra, but passed in the elementary English. (The examination bore down heavily on the "Cotters' Saturday Night," which I had read, for the first time, the night before.) My freshman year was therefore burdened with the need of "making up" deficiencies; I spent my Saturday afternoons with a Latin tutor, to whom I owe much.

Naturally, I had little idea of what I was preparing for, but I hoped to get a teacher's certificate as an anchor to windward, so it was necessary, under the regulations, for me to take two courses in pedagogy.

The pedagogy department, fortunately, was under the guidance of Elmer Ellsworth Brown, of whom I shall speak later. He required of all students in his courses a preliminary course in psychology. Psychology was in the philosophy department, and George H. Howison, head of that department, required a course in logic as a prerequisite for psychology. Accordingly, in the first semester of my sophomore year, I enrolled in logic, with Charles M. Bakewell.

The rapid growth in number of the student body had just begun, and the logic enrollment was too large for class work, hence Bakewell divided it into two sections, one of which was taken over by George M. Stratton, who had just returned to the University from Wundt's laboratory in Leipzig. I fell in Stratton's section. The second semester I enrolled in psychology, which was officially in charge of Stratton; but he divided the class, and Bakewell took one section. I fell in this section. I had the lot, therefore, of taking the logic with an instructor who professed to know nothing of logic, and psychology with an instructor who professed to know nothing of psychology. I think both were reasonably sincere in their professions, but I found both of them stimulating. Both seemed to think I was a mediocre student, and both were correct.

The psychology text was James's *Briefer Course*. I found it dense and unintelligible. I told my troubles to Bakewell, who informed me (kindly but definitely) that the trouble was not with the book, but with my dullness.

Howison lectured once a week to the combined logic sections in the first semester and to the combined psychology sections in the second semester. He was an ex-Presbyterian minister who had become an enthusiastic Hegelian in St. Louis, in company with William T. Harris (later U. S. Commissioner of Education), and had more or less abandoned Hegel for a theological personalism of his own construction. His lectures were powerful sermons, which shook me out of my "common sense" philosophy. I became convinced that there was something in psychology, although I didn't know what.

In cases in the front of Howison's lecture room there were some pieces of psychological apparatus which Stratton had collected as a start towards a future laboratory. I recall a Hipp, some brain models, and nameless other pieces. What they were for I did not know, but they looked promising.

The next year I took a course in pedagogy with Brown. He had taken his doctorate at Halle in history, and was interested (I think) principally in the historical aspect of education. I recognized that here was a scholar, and he was also a man of great personality, himself interested in his students. I became infected with the yearning for research, and have to hold Brown responsible for this.

I took two other courses in the pedagogy department, educational psychology and "ethology," with newer members of the pedagogical staff. The first bored me, and I don't know yet what the second was about. I knew that I did not want research in education; but, in my graduate year at California, I enrolled in Brown's Seminary, in order to work under the man.

I had expected to get a high-school teacher's certificate in mathematics (and I did get it), but I found definitely that I was no mathematician. Trigonometry and solid geometry, with Archie Pierce, I found stimulating. Analytical geometry and calculus (a year of each) I was fortunate enough to have with Mellen W. Haskell, a man of great personality, who made these topics stimulating and alive. There may be greater teachers of mathematics than Haskell: if so, I envy their students. My further adventures in mathematics were terrible. I took analytic projective geometry with Archie Pierce; no textbooks, and his lectures were all given with his back to the class, while making diagrams on the blackboard. Had it not been for the coaching of my good friend and classmate, Arthur King, I should certainly have gone down and out. Arthur was a genius, as his astro-physical triumphs have shown, and his encouragement and help meant much to me.

My other "higher math" course was with Irving Stringham in the logic of mathematics. I seem to remember that Stringham spent one semester in developing a new proof of the binomial theorem, and the other in non-euclidean geometry. No doubt, I was fortunate in my chance to study under Stringham, but again Arthur King pulled me through. I knew that I had gone far enough in mathematics; but I have never regretted the time and energy put upon it. I spent a great deal of time on some other subjects, in which I did reasonably well with greater ease, but which have been of little help to me. I have consistently urged psychology students to strengthen their mathematics, including at least calculus and, if possible, the theory of probabilities. The use of statistical methods by persons ignorant of their foundation and implications seems to me to be a crime.

In my junior year, I took Stratton's course in experimental psychology, the new laboratory having been installed. Here I found what I wanted. Stratton in his own subject was a different man from Stratton teaching logic, and the subject-matter enthused me. The course was of the demonstration type. We all took part in the experiments as a group. We played horse at times, and probably annoyed our instructor; but he was a good sport. I do not think I should have gotten half as much from a "laboratory" course of the type now given in most departments. By the end of the year I knew what I wanted. I wanted to be an experimental psychologist.

In two other sciences I had courses of the lecture and demonstration type. Under Frederick Slate I had heat, light, and electricity—the only course in physics I ever had. Every detail was demonstrated with adequate apparatus—we never touched a wire ourselves—and every demonstration worked. Every time I have to devise a new piece of apparatus I bless "Freddy's" course, and wish my students might have had similar advantages.

One semester of botany (really it was cytology) I had with Setchell: a marvelous lecturer. The next semester (plant physiology) I had with Osterhaut: another exceptional teacher. It was a small class, and microscopes and slides were at hand to demonstrate every point. Laboratory work would have given me far less. I owe an immense amount to these courses in botany and physics.

I had two years of French, at which I worked hard. I mastered the grammar, but got nothing out of the instructors. Both were Frenchmen, probably excellent scholars, but total losses as teachers for American youths. I learned to translate French, not to speak it. At the end of my two years, I could not even read French, but I determined to master that much at least, and, purchasing a two-volume edition of *The Count of Montecristo,* I abandoned grammar and dictionary and plunged in in my spare time. By the time I had struggled through the first volume I could *read it,* and after I read the second volume I went back and read the first. Later, I tried to pursue the same plan with German, but gave it up. I couldn't find a German book of enough intrinsic interest to get me over the top.

In my senior year, I was allowed to do some "research" in psychology. I don't remember what it was, except that I used two terrible spring-driven color mixers, and it couldn't have amounted to anything, but I had a gorgeous time. I was all set for graduate work in psychology. I had taken the history of philosophy with Howison and Bakewell, and was greatly interested in that. Between classes, my classmates and I loafed on the steps of the philosophy building (against Howison's strict orders; he detested step-sitting) and discussed philosophy and psychology. Once a month the Philosophical Union met, and I attended regularly. At these meetings the staff and some townsmen (there was always a large attendance) discussed metaphysical points. My private opinion was (and still is) that most of the discussions were empty sound; but it was stimulating, and, I think, confirmed my tendency to keep my feet on the ground.

I also had a course in Lotze with McGilvary. He was at the time an enthusiastic Hegelian, and radiated force and energy in his class. I don't remember anything about Lotze; but I do remember that I was interested, and did a great deal of thinking. After McGilvary left California, and lost his Hegelian faith, he became perhaps a sounder philosopher, but a less stimulating one. He could do great work with his students because of his enthusiasm. I wonder if it matters much what the philosophers teach if only they know the subject and are fired by it. McGilvary scored on both points.

I did one graduate year at Berkeley. During this time I had more personal contact with Stratton, whom I came to respect and admire more and more. I found him surrounded by a shell of reserve, through which I had to break, like a burglar who breaks into a house and receives warm and hearty welcome when he is in. I wish more of his later students had been burglariously minded.

During this graduate year I worked out my first real problem: *The effect of imperceptible shadows on the judgment of distance.* This problem was supposed to bear upon the question of the subconscious, then beginning to exercise American psychology. I still think it an important piece of work, and wish it might be repeated with better apparatus. My own point of view was against the thesis, and I can remember my disappointment when I found that the imperceptible shadows *did* have an effect. In publication, the editor of the *Psychological Review* managed to interchange the numbers of the cuts, so that the statements in the text were at variance with what the cuts showed. Although this interchange was noted in the next issue, many readers never had their impressions corrected.

This work was ostensibly repeated later in Titchener's laboratory, with negative results. Really, what was repeated was my first attempt, with blurred shadows, which I had found not satisfactory. The work from which my conclusions were drawn never has been repeated.

I worked on a number of minor problems besides the imperceptible shadows. I had the run of the laboratory, and tinkered with apparatus to my heart's desire. Some of my catastrophes probably never were known to anyone but myself (but I cannot be sure, for Stratton was a wise man, and knew when to appear unwitting). I can think of no better method of training a novice than that which Stratton employed.

I had another advantage in this year: Montague came to Berkeley, fresh from Harvard, and gave a course on Kant. We read all three critiques, nay, we studied them, and wrote papers about them. We couldn't "get" Montague personally at first. Probably because he was an odd figure, constantly wearing the same loud golf costume everywhere—and knickers were not a common sight in California at that time. Not until some years later did I discover the reason for Montague's sartorial peculiarity. The Southern Pacific had lost his trunks, and for weeks he had no other clothes to wear except the golf suit he had worn on the train.

We did become "sold" on Montague in time. We found him a man it paid to disagree with, because he would discuss mooted points in a sympathetic way, and seemed to deem no serious student too poor a fish to labor with. I find now that, when he comes to Baltimore, my students approve of him, and profit from him, in the same way we used to do.

The next year I went to Harvard. Stratton had just criticized Münsterberg's *Psychology and Life* and had been contemptuously rebuked by Münsterberg for his irreverent presumption. Still, he advised me to go to Harvard, and gave me a warm letter to Münsterberg. At Harvard, Münsterberg was in his full glory, Royce was at his peak of fame, and James was just ceasing work. I listened to some of Royce's lectures, and attended one session of his seminary, and decided that I didn't know what Royce was talking about, and didn't much care. I had the same experience later with John Dewey, whom I heard first at Harvard, and later more extensively at Johns Hopkins. I found these philosophers sounded well, until one began to inquire what their phrases meant, when translated into terms of everyday life. I decided that if what these philosophers were preaching was important, then I was going in for unimportant things. (Maybe I did.)

Palmer—Phil 4—I found most entertaining and interesting, but the class (mostly undergraduates) interested me still more. Palmer had the effect on them which a good revivalist has on his flock; they sat up on the edges of their seats and thrilled to his words. It was all good common sense, too. The next year, I took Palmer's seminary—Hegel's ethics, I think it was—and liked Palmer immensely, personally. As a mental stimulus, however, the seminary was a wet poultice.

James was in Italy during my first year, but, in my second, gave his lectures on *Varieties of Religious Experience.* He was a poor lecturer, passing hastily over his best points. But it was worth something to see him who was then the psychological hero. I had no chance to become personally acquainted with him.

My real work was with Münsterberg, or strictly speaking, under him. He assigned me a problem in which I had no prior interest, and told me that if I needed apparatus to apply to Robert McDougall, then Instructor in the Department. I floundered badly, but finally devised a method of attack on the problem. There was no apparatus available, but I constructed some myself, and had some small pieces built in Boston. The apparatus was highly inaccurate, and my methods were not much to brag about. Münsterberg would come into the laboratory daily, and, slipping on his old velveteen coat, make the rounds. If I was "experimenting," he would quietly open the door, stand a while watching me, and then step out, and that was that. I suspect that many times he was thinking of something else as he stood there. One day, however, when I was alone in the room, and adjusting a mercury-contact pendulum which I had built the night before, he said, in his inimitable dialect, "Where did you get that? I have not seen that in the laboratory before." "Oh, I made it, Dr. Münsterberg," I replied. "Well," he said, "I could write a book of 300 pages easier than I could make that" (but he said "zat"); and went out.

Of course, we could have had more personal contacts with Münsterberg by making an appointment for a conference. But that was a formidable undertaking and seldom done. At his home, where he sometimes had us in a group, he was affable, entertaining, delightful.

In his seminary, he was at his best, and there we got the meat of our work. Never did any loose conclusion or faulty method get by him. I well remember a paper I prepared, which I thought rather good. It was a mass of shreds when he got through with it, and I was perfectly convinced on every point he made.

I got no teaching (aside from the seminary), and I got no direction in my laboratory work or my reading. Whether this was good for me or not, I am yet uncertain. I came away knowing little of the detailed subject-matter of psychology as it was then conceived. Perhaps this was a blessing, as I have had less to forget.

From the group of graduate students I received much. Especially from Angier, Yerkes, and Holt (the latter was an instructor in my second year). We were all drawn to Harvard by the same force—it was *the* center for psychology at the time, and, in spite of the informality and laxness of instruction, we were confirmed in the scientific path. I never understood Münsterberg's philosophic basis; he himself told me that nobody did; but that didn't matter. He radiated scientific impulses, and profoundly altered the course of American psychology, even if not into the exact course he would have chosen. There was at least one giant in these days, although his publications do not show it. Münsterberg made no converts to his philosophy. He was not a man to have disciples. But I think the modern trends in American psychology, and especially its experimental interests, are easily traceable to Münsterberg as their father.

My graduate work done, I returned to California, and spent four years as assistant and as instructor. I admired Howison and enjoyed companionship with Montague, and was in a position to gain much from Stratton. But, in spite of them, the four years were unprofitable. I taught, and what I taught was the orthodox stuff. Howison was autocratic, and really had little opinion of psychology. I got nothing done, and made little advance. Conditions were probably as bad in other places where psychology was under the thumb of philosophy. The debâcle at Harvard after Münsterberg's death is merely another illustration. The intellectual atmosphere at California was undergoing rapid changes. The old stimulating air of scholarship and ideals was on the chute. The coming of Wheeler and Morse Stephens produced in no subtle way an entire change of front. Students were openly told that they did not come to college for the curriculum and the chance to study, but for the social advantages, "to be men among men." Numbers, "results," and popularity were the ascendant ideals. The old-style, detailed scholarship that got no results to immediately impress the public was not encouraged. It was a good place to get away from, and it required many painful years for the institution to recover its lost ground.

At the end of two years, Stratton was called to Johns Hopkins, and conditions in the department at California were, of course, worse. My life was saved two years later when Stratton invited me to assist him at his Eastern post.

Conditions at the Hopkins were, however, by no means ideal. J. Mark Baldwin was Professor of Philosophy and Psychology, and Stratton was Director of the Psychological Laboratory, but under Baldwin. Baldwin was in the midst of developing his philosophy, and the students in the Department were in the main not very serious. Only one man (Trigant Burrow) was majoring in psychology. Our quarters were inadequate in size, and what space there was was difficult of utilization. E. H. Griffin was Dean of the College, and gave courses in the history of philosophy. There was a prescribed undergraduate course, of which the first trimester of the year was directed to logic, the second psychology, and the third ethics. It was not a success. Baldwin came to the Hopkins in 1903, Stratton in 1904; up to 1909, two doctorates were awarded in philosophy, and none in psychology.

My duties were not onerous. I had plenty of time for research, but I had difficulty in getting under way. I had just married, and was in dire poverty. I suspect Stratton was deeply discouraged in regard to me. (I know he was discouraged in regard to the general situation.) I was discouraged with myself. A part of the plan in bringing Baldwin to the university was to inaugurate education. Baldwin suggested to me that I take up that line. I was tempted, but decided to stick to the line I was making a mess of. I suspect it was just as well.

Early in 1908 President Wheeler stopped in Baltimore on an Eastern trip and asked Stratton to return to California. He accepted. If he had stuck it out a year longer here, he would have been over the peak. Stratton's leaving completed my discouragement.

I was, however, beginning to find myself. I had profited a great deal by Hopkins association, and Stratton had done everything for me that he could do.

The university had not at that time begun its disastrous series of steps to cater to "local needs," which began with the institution of courses for local teachers and which has recently resulted in a School of Higher Studies in Education. It is significant of the real source of trouble in higher education that the successive five or six steps of this sort were taken by the Board of Trustees without consulting the Faculty, who were informed after the damage had been done. It is amazing that men who would not think of permitting revolutionary changes in their own business, without the consulting of those who really know the details of the business, should act in an entirely different way in one of the most important businesses of the country. But, then, I suppose that in commercial affairs it is realized that there is in every case a product to be considered, whereas trustees, in general, look on university work as a business without a product, or estimate it innocently in terms of the number of degrees conferred.

I was beginning to develop some experimental problems of my own, and I was rapidly becoming radical in my thinking, and throwing away the old phraseology and the old conceptions. The seeds planted by Münsterberg were sprouting. I was deciding that psychology was, after all, a practical subject, which could be aligned with "common sense," and the natural sciences; and I was beginning to cast about for ways in which it could be based on the methods of general science. I no longer believed in the old gods of introspection, consciousness, and sensation, although I could not find as yet, and did not find for a long time, the proper terms for the formulation of the new conceptions I was fashioning. "Functional psychology," then popular, left me cold because of its nebulosity. If the term "functional" had not been taken by this school, I should have adopted it with another meaning. I have no objection to being classed as a "functional psychologist" now, because "functional" has lost the artificial significance of "function of consciousness" which it had at that time, and now is understood to refer to functions of the organism.

On Stratton's going, the worst I feared did not happen. Instead, Watson was brought from Chicago, and brought into the Department a spirit of energy. As a laboratory director, as well as in our personal relations, Watson was admirable. There was never any hampering of my activities, or any dictation. I only hope that Watson did not find my presence in the laboratory less pleasant than I found his.

Watson, at that time, was primarily an experimentalist. He built apparatus with his own hands; he got the university interested in the laboratory. He stimulated me to put through some of the things I had been maturing.

I had already discarded the old doctrine of "images." Watson, however, still accepted it. He, he said, used visual imagery very effectually in designing his apparatus. Watson had not at that time developed his behaviorism, and his thinking was, to a large extent, along conventional lines. He was violently interested in animal behavior, and was looking for some simplifications of attitude which would align that work with human psychology. Hence, he was interested in the iconoclastic activity I was developing, and was influenced by my views, but carried them out to extremes. I rejected images as psychic objects, and denounced introspection as held by the orthodox psychologists. Watson carried this further, to the excluding from his psychology of everything to which the word "introspection" could be applied, and excluded imagination along with images. I had questioned the possibility of observing "consciousness." Watson carried this to the extreme, also. His first behaviorism, however, was obviously based on the orthodox system by which the mental field was divided into perception, thought, and feeling; and he was merely finding physiological substitutes for these. When I called his attention to this, and urged him to study behavior as behavior, he admitted the apparent Titchenerian basis, but opined that he could get away from that in later writings. He did, eventually, but only after the American psychology generally had moved ahead.

In 1912 I published my first book. In this I leaned over backwards, partly because I feared that it would be useless as a text if I was too radical, and partly for lack of formulation and terms to present the point of view towards which I was tending. Hence, it never represented my actual state of progress, except in a few particulars.

I could not, for example, deal with the vital topic of imagination in such a way as to indicate the reality of the imaginary process, and yet discard the psychic-object image. I realized the essentiality of the reaction, and the integration action of the brain; but found the "reflex arc" hypothesis too artifical and limited, and was troubled by the fact that there *were* actually sensory and motor centers, which must be distinguished from the centers as traditionally conceived. I realized that no sensible meaning of "consciousness" made it an *object,* as current psychology did, and yet the term stood for an inescapable fact. I was not yet in a position to solve the puzzle. On the other hand, I naïvely accepted the orthodox view of instincts, though I did not base my nascent system upon them, as Watson did his first behaviorism.

In respect to the error in regard to the interpretation of behavior, it is possible that Watson's productions made clear to me the impossibility of development in the behavioristic direction. Be that as it may, I never inclined in that direction. I have always felt that behaviorism held back the development of scientific psychology, not only because its exaggerated point of view aroused antagonism against real progress, but also because it confused a large number of young minds who adopted it because of its novelty and seeming simplicity. The real trouble with behaviorism was that it was a step back, being more closely allied with Titchenerism than with the scientific psychology which was emerging. Probably most of those who call themselves behaviorists today are quite unaware that progressive psychology of the old line has dispensed with the kind of consciousness and introspection in which the behaviorist is analytically interested—a program to which behaviorism has contributed nothing.

In 1909, J. Mark Baldwin left the University to take up residence in Paris. This led to the calling of Arthur O. Lovejoy to the Chair of Philosophy, and shortly to the division of the department into three independent departments; philosophy, psychology, and education. The stimulation which Lovejoy has given to staff and students in psychology, not only through courses, but in informal colloquia and contacts, has been of great benefit. An ideal situation for the development of sound psychological methods is that in which there is intimate relationship with an independent department of philosophy headed by a critical, keen, and impartially minded scholar.

In 1916 a serious crisis in the Department occurred. The philosophical faculty, as the non-medical division of the university was called, was about to move to a new location. In its chronic state of poverty (as respects the philosophical faculty, that is; the medical school, of course, has fared more richly), the University was not able to provide adequate space. There was nowhere for Psychology to go. At this juncture, Adolph Meyer invited Watson to move to the Phipps Psychiatric Clinic, where a few rooms for a psychological laboratory had been supplied. Watson decided to go, but there did not appear to be sufficient space for my work also, and I could not see the possibility of carrying it on if separated from the scientific departments. I found an attic room in the new Gilman Hall, and President Goodnow was willing to have it divided by wooden partitions into small rooms, with sloping ceilings. So the Department bifurcated, Watson going to the Hospital, and I to Homewood. In effect there were two one-man departments, but each had space superior to the old premises.

I had been ten years in the old laboratory at Eutaw Street and Druid Hill Avenue. Now I commenced what turned out to be a new ten-year period. A few graduate students appeared—English Bagby, and Schachne Isaacs among them. There seemed to be a real opportunity for work—and then we entered the War.

Watson was drawn into the Air Service, and assigned to the Medical Research Board. One of the problems of this board was to rate aviation candidates and fliers in respect to their endurance of the reduced oxygen conditions of high altitudes. The utility of this work seemed questionable (I don't think Watson thought it worth while), but the Board were determined to do it. Physiologists were put at work, and devised means for progressively asphyxiating victims, but could get no further. Reduced in oxygen supply to a certain degree, the victim would faint, and that was that. The determination of the earlier effects seemed to be a job for psychologists, and Watson sent for me, and asked me to undertake the job. I was dubious, but was promised an opportunity to work on the more vital psychological problems of aviation, and the bait worked. I entered the work, and was after a while commissioned. I took English Bagby and Schachne Isaacs in with me and we attacked the job.

I was anxious to avoid having the victim faint during the asphyxiation test, and urged the taking of the victim's blood-pressure during the asphyxiation, as a precautionary measure. The physiologist in charge decided against this, on the assumption that blood-pressure had been shown by the work on Pike's Peak to have no consistent relation to oxygen shortage. I therefore, with Watson's authorization, put Bagby to the job of taking blood-pressure of the victim during the process. Bagby was able to take systolic readings every second minute and diastolic in the intervening minutes, with fair accuracy, and the first few cases showed a beautiful curve which proved our point. The physiologists thereupon took over the blood-pressure determination, to our great relief.

For the psychological determinations, we used, of course, the available psychophysical tests, and devised new ones. But none of them showed anything. The patient, when slowly asphyxiated during 18 to 20 minutes, showed his normal efficiency up to a few seconds before the point at which he would faint if not given oxygen. This did not look right. It was Bagby, I think, who found the key to the puzzle. The tests were all too brief. The patient could maintain a level of efficiency for the requisite time, and, when the task was over, slumped mentally. What was needed was to keep the patient at work continuously, and yet to be able to estimate his efficiency at any minute. With the definite problem in view, we set to work, and devised the "L.V.N." test, which need not be described here. The work had been started in Washington, but was transferred to Mineola as soon as quarters were ready.

At Mineola, there were many aggravating conditions, but we perfected the test, and trained a number of psychologists to make the observations. Under the conditions in which the determinations were made, graphic records were impossible, but the "clinical" method proved surprisingly reliable. Indications of inefficiency could be obtained early in the asphyxiation process, and there obviously were individual differences. Whether these differences were of permanent sort, or due merely to the temporary conditions of the patient (consequent upon fatigue, indigestion, preceding sprees, etc.), we did not know. I proposed next to investigate this point, but we were not allowed to do so. We had a test: the test "worked": it must be applied. As one all-too-influential medical officer pronounced: "We have no time for pure scientific work; this is *war!*"

The Medical Research Laboratory of the Air Service was in charge of Colonel (later Brigadier General) Wilmer, whose sympathies were entirely on the side of sound work. Unfortunately, Colonel Wilmer's hands were tied, the Air Service being strongly influenced by a small group of medical officers of no scientific ability, and interested only in building a large organization and elevating themselves.

There was no help. We spent our energies in building apparatus to equip field units, and in training psychologists to make the determinations. These psychologists were sent out with units to the flying fields, and many aviators and candidates were "tested" and "rated" by tests which probably had no practical value whatever. I do not know how much money was wasted, and how much the actual aviation work was interfered with by these tests and the physiological tests which went with them. The results of our work have been scientifically valuable, but that didn't help the war any.

The psychologists were not allowed to get into any of the really interesting psychological problems of the aviator. But worse was to come. The "rotation tests" for determining the "equilibrium" of candidates for aviation had become a scandal. These tests had been promoted and exploited by the small medical group above mentioned, although the more competent otologists charged with their application were by no means convinced of their validity. In the earlier days of their application, candidates were turned down because of their rating in this test, and certain of these became successful fliers in foreign forces. Finally, it was found that some of our best fliers could not pass the test.

The situation being scandalous, Colonel Wilmer ordered me to investigate the tests to determine their validity. My first step was to detail Captain George R. Wells to determine the nystagmus of certain trapeze performers and whirling dancers available in New York, and who were obviously efficient in equilibration. Tests were made, the acrobats being rotated and their nystagmus timed by an otologist of experience in this work (he had applied the tests in one of the examining boards). The results showed that, in most cases, the nystagmus duration was reduced below the officially "safe" limits; in other words, these acrobats would have been turned down as candidates for the aviation service on the score of their lack of equilibration!

The data on these tests were in Captain Wells' possession, and were immediately demanded of him by a superior officer. Wells unfortunately had no copy. The data were officially published later, with the figures changed to make it appear that the acrobats "passed."

Wells was immediately ordered out on a "field unit." I therefore assigned the next job to Madison Bentley, who had just been assigned to our laboratory. Bentley had the advantage of being commissioned in the Air Service, to which he was directly responsible. He carried out some excellent and illuminating studies in New York on whirling dancers, and he put through the important experimental test which had been contemplated for Wells. Six enlisted men, "passed" in the physical examination (including the rotation tests) for candidates for the aviation force, were selected. The physical examinations were given by officers experienced on the medical examining boards, and of unquestioned ability. These men were rotated for from 15 to 20 days each, at the rate of one revolution in two seconds, each man being given only ten turns on each day, five in each direction (a total of 20 seconds of "practice"). The post-rotation nystagmus of the six men was between 23 and 30 seconds' duration at the beginning. At the end, it lay between 6 and 13 seconds, and no other effects on the men could be observed. The carrying on of these experiments, of course, excited great opposition. The results were terrifying to certain of the powers that were. The results, published in the *Manual* of the Medical Research Laboratory (1918, Government Printing Office) were accompanied by an "editorial insert" attacking them. (The "edited" results of Dr. Wells' tests on whirling dancers and acrobats were also published in the same volume in another place.)

We in the Psychology Division were, of course, somewhat naïve, assuming that those promoting the "rotation test" were merely uninformed concerning the phenomena. Not until the episode was entirely over did evidence come into my hands that the situation was somewhat different: that candidates reported by different examining boards with "nystagmus" far below the standard requirements, but otherwise physically normal, were being accepted, and nothing said about it. Whether the actual nystagmus durations reported by the examining boards were filed in Washington as sent in, or "corrected," I do not know.

A way was shortly found to handle the uncomfortable situation. A unit from the Medical Research Laboratory was to be sent to France, headed by Colonel Wilmer. I was at first designated as a member of this unit, but, shortly before it sailed, and after I had made preparations, at considerable expense, the order was countermanded, over Colonel Wilmer's recommendation. After Colonel Wilmer was out of the way, things began to happen. Watson and Bentley were ordered back to the Air Service in Washington. I was transferred to Washington without any work to do. I was warned by close friends that it was hoped that I would get restless and "go overhead," thus giving occasion for definite procedure against me. (Charges that I had "gone overhead"; as well as other and more scurrilous charges had been circulated, but, of course, these concoctions gave no grounds for procedure.)

In Washington, I found that Major Roy J. Pearce of the Chemical Warfare Section in Cleveland wished the assistance of a psychologist, and I requested and received the assignment. Pearce was an energetic man, and it was a pleasure to work with him, although I fear I contributed little. The flu laid me low for five weeks, and I was in the hospital on Armistice Day.

The work of the Psychological Division of the Medical Air Service might have been of great importance. There were, of course, many psychological problems involved in the training of fliers, but these problems were put into the hands of otologists, gynaecologists, and other medical specialists, and the psychologists were not allowed a look-in. The turmoil over the nystagmus situation, accentuated by the events on Long Island, stimulated post-war work on "vestibular functions" very markedly, and the work of Griffith, Holsopple, Dorcus, and others was directly due to this. Physiologists, also, were stirred up, and the volume and quality of physiological and psychological work perhaps justifies the efforts which Watson, Bentley, Wells, and myself made.

The low-oxygen work, also, was productive, although the usefulness for war purposes was nil (as I had expected). Utilization of the results is slow, but we are now in a position to produce experimental measure of fatigue and drug effects impossible before the War. Records which we were collecting in the Division, but which disappeared, apparently between the time of my leaving and the coming of Dr. Stratton to take charge, would have been exceedingly useful in promoting this work. However, several pieces of apparatus on the L.V.N. idea, but improved for experimental purposes, have been produced since the War, and more work will undoubtedly be done on this line.

In 1918, Watson resigned from the Johns Hopkins, and entered the advertising business which he has pursued so notably since. His decision was suddenly made, and was a distress to me for a number of reasons. The spatial division of the Department had been somewhat awkward, but otherwise the Department had been getting rapidly on its feet, and the future was beginning to light up. Our quarters, together, were reasonably adequate, but my quarters alone did not seem sufficient housing for the Department that could be forseen. Moreover, although Dr. Buford Johnson had been added to the staff, Watson's resignation left it, of course, inadequately staffed.

It was especially unfortunate that Watson's work on infants was interrupted almost at its inception, since I believe that in a little more time he himself would have seen clearly that there was no more reason to apply the words "fear" and "love" to the infant's simple responses, than to apply the terms "pain" and "pleasure" to the same responses. This unfinished work has, unfortunately, had a very confusing effect in the field of child psychology since, because the public has attended only to the tentative form of the conclusions, not to the details of the work itself.

The rapid growth which I had foreseen in the Department occurred as per schedule. Graduate students steadily increased in number, until at the present time the problem is one of exclusion, and the demand for undergraduate work (I had had psychology removed as a prescribed course on moving to Homewood) also grew apace. The attic quarters in Gilman Hall became so crowded with students and apparatus that it was necessary for much work to be done at night, and for the last two years there, the laboratory was seldom untenanted between the hours of 8:30 A.M. and 2:00 the next A.M. We had reached, by 1926, a condition of absolute saturation, and it did not seem possible to continue either graduate instruction or research.

President Goodnow, however, unexpectedly came to the rescue. The most gratifying information I have ever received was his statement that he had been noting our heroic work under extreme difficulty, and had determined to give us relief, if possible. The relief he suggested was the moving of the laboratory into the vacant "hospital for crippled children," which stands a little distance from the campus, and which was the property of the Johns Hopkins Hospital. This seemed to me like a new lease of life, and the arrangement was made with the Trustees of the Hospital. The agreement, however, was a contingent one, and ended in 1931 when the U. S. government commandeered the site.

With a relatively little fixing-up, the "new" laboratory afforded excellent accommodations. In the smaller building attached to the laboratory Dr. Johnson organized the Child Institute, providing for 30 children between two and five years of age, and providing splendidly for training of workers with children and for research. This Institute is amazing, in that it was organized on a shoestring, has had but a trifle of financial support in addition to the building, and stands in the front ranks of Institutes of this kind, a monument to the ability and devotion of Dr. Johnson.

The organization and equipment of the laboratory itself was delayed somewhat by my own absence for one year on full time as Chairman of the Division of Anthropology and Psychology of the National Research Council. During that year, my time was fully occupied by the heavy work of the Division, and the Department in Baltimore was administered by Dr. Johnson. The next year I continued on half time on the Washington job, to finish, if possible, work which was gotten under way the preceding year, so that the Department here still suffered.

During the tenancy of the Gilman Hall attic, I was able to start research of a number of lines, but not to finish them, on acount of the cramped conditions. These now began to be taken up—some of them have already—by myself and by research students, and, with good luck, five more years will see a clean-up. The Hopkins chronoscope was devised and made before the War. Many new pieces of apparatus have been made and tested since then, and a number partly designed can now be brought to completion. The success in this work has been due in no small part to the skillful work and interest of Charles M. Childs, Chief Mechanician in the physics work shop. My relations with him have been among the most important of my professional contacts.

Psychobiology was established in 1917. I founded this journal at a time when publication seemed adequate for psychology, but at which I was able to foresee certain important changes which made the establishment necessary. I shall not go into these details. The merging of *Psychobiology* with the *Journal of Animal Behavior* into the *Journal of Comparative Psychology* after the War left me with the brunt of the editorial burden. The establishment of *Mental Measurements Monographs* has not added much to my responsibility, but the taking over by the Johns Hopkins Press of *Comparative*

Psychology Monographs has. The burden of editorial work, in addition to heavy departmental duties, would not have been possible without the splendid relations established with the Williams and Wilkins Company, whose President, Mr. Passano, is able to make even editorial work a pleasure. The good nature of contributors has also been a great aid. Psychologists, at least, take suggestions and strictures in the best of part, and cooperate with editors when given a chance. The tolerance which members of editorial boards show towards managing editors is also remarkable, and soothing.

There is a great danger in psychology (and I assume in other subjects, too) that publication may fall into the hands of a coterie who will promote certain lines of work while turning a cold shoulder to others. For this reason, the gathering of certain publications into the hands of the American Psychological Association is useful (except for the expense), so long as there are sufficient numbers of independent journals. The establishment of the Clark group of journals was distinctly advantageous, and the independence of the *Journal of Comparative Psychology, Comparative Psychology Monographs,* and *Mental Measurements Monographs* is highly important.

My editorship of the *Journal of Comparative Psychology* was looked upon as temporary, and I am still looking forward to early release. Editorial work is a contribution which is made by institutions and by universities who pay the editors' salaries, and which ought to be distributed, as the interference with other activities is serious. I am not a comparative psychologist (in the practical American usage of the term, which means *animal psychology,* although it should not), but there is a distinct advantage, at times, in having an editor who does not know much about the subject and has no particular bias. There is always criticism of the editor for not excluding certain manuscripts deemed below par in scientific value: but I have found that the strongest criticisms are likely to come from those whose own contributions are seriously objected to by others. No editor is omniscient, and articles which seem not important today often turn out in a few years to be highly important. The reverse is also true, hence a considerable catholicity in selection is essential.

My assuming the editorship of *Comparative Psychology Monographs* was largely due to a miscalculation on my part. Hunter having resigned the editorship, the Editorial Board (with which I was in no wise connected), in amicable arrangement with the original owners, the Williams and Wilkins Company, asked the Johns Hopkins Press to take the series over. I acted as intermediary in the arrangement, assuming that an editor would be found elsewhere, and then found that the Johns Hopkins required a responsible editor on the ground. The emergency has been taken care of by making the Editorial Board maximally and specifically responsible for the acceptance of manuscripts.

Psychology Classics were established in the hopes of making more available materials not hitherto translated, or out of print. This has slowly gotten under way, but the chief difficulty is in securing translators. At present, as the books most desired are not popular and translators, therefore, cannot be compensated, we can expect to find few who are able to devote time to the work. Provision of fellowships, to be occupied by psychologists willing to take a semester or longer from their regular duties to complete translations in which they are interested, is vitally needed.

The reference above to the American Psychological Association needs further extension. The Association today is in a peculiar state, with a large and miscellaneous membership, and relatively few psychologists have confidence in it as a controlling organization. The particularly undesirable feature is in the present election system, which was introduced with the highest of motives, namely, to make the control democratic. It has, however, worked in an unexpected way, one effect being that the two candidates for presidency on the final ballot are really selected a year or two in advance by the vote of a relatively few members. The "runner up" in any year is certain to be on the final ballot the next year, for obvious reasons, and a new "runner up" seldom receives more than forty votes on the first or nominating ballot, the great majority of the "nominating" votes being widely distributed.

My work as Chairman of the Division of Anthropology and Psychology of the National Research Council may or may not prove to have been useful to psychology. A large part of my energy was expended with two large conferences and many committees in the promotion of plans for research on problems of the deaf and the hard of hearing. In this I received the strong cooperation of psychologists, physiologists, anatomists, otologists, and educators of the deaf. I believe that everything was done that could be done prior to the securing of ample funds for the prosecution of research, and that the *Research Program* evolved and published recently as a *Bulletin of the National Research Council* covers the field. Of a purely anthropological nature, the Conference on Midwestern Archaeology held in St. Louis may prove to have given valuable impetus to the work of conserving and studying the sites and materials of the valleys of the Mississippi and its tributaries. The Conference of Editors and Business Managers of Anthropological and Psychological Periodicals accomplished something, but it is too early to tell how much. In the same way, the conference on racial problems, held jointly with the Social Science Research Council, waits for its work to follow it. The Carlisle Conference on Experimental Psychology has already had two important results which can be definitely indicated, as well as several useful minor results. One of these major results was the subsequent organization of the National Institute of Psychology, which is as yet in its swaddling clothes. This organization has been violently attacked through private communications in which the main objections seem to be that it was not "authorized" by either the A.P.A. or the N.R.C., neither of which, as a matter of fact, could legitimately "authorize" it, although the Division of Anthropology and Psychology definitely approved the basic plan, namely, the establishment of a National Laboratory, but never expected to be responsible for the financing and maintenance. The National Research Council regards the establishment of the Institute as a legitimate outcome of normal Council activities, the uniform purpose of the Council being to assist in the organization of worthy scientific enterprises, and to expect them to cut the leading strings as early as possible. Working relations between the Institute and the Council will be established without difficulty. I suspect the real objections to the N.I.P. are, first, the exclusiveness of the organization, since it does not admit members in the fields of educational, vocational, industrial, clinical, social, or other than "experimental" psychological research; secondly, the age limit which relieves members over sixty of voting and office holding; and, thirdly, in its actual democratic type of organization. Changes will doubtless be made in details of organization and control when the time comes (the initial and somewhat arbitrary by-laws obtain for one year), but the Institute is a healthy youngster, needing now for the members to take charge, and large funds for research. The need for this organization, and for its independence, is generally recognized. At the present time we have no comparative psychology, although we talk about it. The development of a real comparative psychology needs the organization of national laboratories where a body of men shall work cooperatively on a variety of animal and human problems and in intimate relation to other institutions. Meeting for discussion is useful, but *working* together is the prime requisite. Abnormal psychology needs to be put on an experimental basis, and much more coordinative work is needed by an organization which is not controlling or directing research but actually doing it.

Psychologists of my generation have seen the growth of psychology from its infantile stage into husky (and sometimes ill-coordinated) adolescence. The present stage is exasperating enough, but we have merely to look back thirty years to be greatly encouraged. I studied, as a textbook, James. I read Titchener, Münsterberg, Külpe, and other textbooks. I taught James, Maher, Wundt, Stout, Angell, Pillsbury, and perhaps some others I have forgotten. Looking into these texts is like going into an archaeological museum. They have done their work, and we have shelved them in an honorable retirement. The questions concerning introspection as present observation or memory; interaction *vs.* parallelism; two-level or multi-level attention; bidimensional or polydimensional feeling; and a host of other "problems": who is willing to discuss these now?

Some of our changes have been less fortunate. Images have almost disappeared from the texts, and the determination of image types has ceased. But, unfortunately, the vital topic of imagination has been ignored. Few study "states of consciousness" any more; but many blunder into the study of (or into words about) the "unconscious." The mythological instincts have dropped away; but an equally mythical intelligence (or intelligences) has raised a ponderous head. The over-preponderance of threshold methods has ceased in our laboratories; but we are in grave danger of not insisting on thorough training in these inexorable fundamentals. We have gone a long way in thirty years, but we have a great distance of travel still in sight before us.

Many byways have been traversed, by certain groups at least, with little profit. In addition to the detour of original behaviorism, lanes labeled *"Aufgaben"* and *"Bewusstseinslagen"* have attracted the feet of some. "Gestalt" now branches off to the left, and "personality studies" to the right; but the moving caravan is becoming wary of detours, and these routes attract only the stragglers, as the caravan becomes more and more consolidated and unified.

The astonishing thing clearly seen in retrospect is that, where we apparently have made our greatest breaks with the past, we have actually honored historical consistency the most. In casting overboard the most confusing accumulations of theory and interpretation (for which we may hold the Germans largely responsible), we have based our progress more solidly upon the methods and practical aims of the German laboratories. If we consider the long line of researchers upon reaction-times, perception of space and time, memory and learning, and all the other studies of performance (not behavioristic, but studies of conscious responses) initiated a generation ago, we realize that our reconstruction and reformulations are but preparation for the more vigorous attack on these problems, and, in improving on the methods where we can and rejecting the interpretation for the most part, we are but the more honoring the sturdy pioneers who filed the claims on the territories we now occupy.

Among the strongest influences on my thinking and professional activities, I count Janet; but there are some earlier French alienists who introduced me to many points, including the sexual theory of the neuroses. I shall not list their names, as I do not at present remember which point I gained from which. John Stewart Mill, Reid, Stewart, and Destutt de Tracy I count among my spiritual ancestors. Descartes (especially in the *Traité sur les Passions de l'Âme*) and Spinoza have been exceedingly suggestive and stimulative, and I hereby thank Dickinson S. Miller for drumming them into me at Harvard. I *think* I got a great deal from Kant (but, then, who can tell what he gets from Kant?). Hegel, Leibnitz, and the modern philosophers left me cold. In social psychology, Westermark and Robertson Smith have been the brightest stars in my firmament, with Budge, Rhys, Davids, Max Müller, and a number of others shining in different magnitudes. Tarde, Le Bon, Spencer, and Ross have bored me to an extreme. McDougall and Graham Wallas have irritated rather than stimulated me.

In experimental psychology, I think that two pieces of work by Stratton have stood out as most illustrious examples: his experiments on inverted vision and on eye-movements. Franz's work on brain localization and on re-education have seemed models, also. None of these have I supposed to be entirely adequate in execution, of course, but the grasp of the problem and the conception of the attack merit a respectful consideration. These, with Ebbinghaus' work on memory, have seemed to me to be striking models, to measure up to which is a worthy ambition, to surpass which is a high success.

My own contributions I should put under three headings: theoretical, experimental, and instrumental. Of the first, I find, in looking back over the recent past, the ones which seem to have had important results are as follows: (1) The attack on introspection (although my most important article, to which the first is only an introduction has, I think, not yet been fully appreciated). (2) The insistence on response or reaction as the basis of mental processes, including thought processes. (3) The attack on images and the insistence on objects of perception and of thought, in general, as real objects, instead of being "psychic" objects, "states of consciousness," or "sensations," and "images." (I suppose I align myself with Brentano on this point, although I confess I have never read him.) (4) The emphasis on the periphery, in general, instead of on the brain, as the fundamental determiner of psychological qualities. (5) The elimination of "instincts" and the emphasis on desires. (6) The view of consciousness as an abstract reference to an undeniable fact of response. (7) The reformation of our views on heredity.

I confess I still think my "Theory of the Syllogism," buried in the first volume of the *University of California Publications in Philosophy,* a real accomplishment, aside from its being an integral part of my "scientific" program. My identification of pitch with extensity is still an orphan in a cold world, but I have hopes for it. I was the first to demonstrate the "practice effects" in group intelligence tests, having undertaken the experiment immediately after listening to a public statement by Thorndike that there were none such. My method of obtaining the Mean Variation directly from the items without the individual subtraction is apparently accepted as standard.

There are not many further propagandist efforts in which I am interested. The abolition of "emotions" and the attempt to get American psychologists generally to understand the concept of the "unconscious" and its implications, I shall probably continue, and I expect to put some efforts on the development of my proposed "reform" in regard to habit formation, but more particularly in its experimental aspects.

As to my own experimental work, aside from what my students have done for me, I should rate highly my demonstration of the relation of the "complication phenomena" to synchronizing reactions of other sorts; my demonstration of the "central" process in visual adaptation; my proof of the relative unimportance of the "eyes" as compared with the "mouth" in emotional expression; and my demonstration of palmesthetic pitch perception and of palmesthetic beats and difference tones. It is true that my solution of the complication problem is far from final, but the "prior entry" hypothesis is definitely "out."

My work on subliminal shadows is important enough to be followed up. My indication of constant errors in auditory localization has been overlooked by others, along with the demonstration of the inaccuracy of the conventional "sound cages," but I believe them to merit further attention.

On many other points I have made mere beginnings, and hope to work them out more fully if granted time, free from administrative duties, between sixty and seventy. My organization work in several of the fields of social psychology I hope to have completed within the next few years.

My apparatus work has resulted in the production of the synchronous-motor chronoscope, and, recently, in an improved set-up of accessory apparatus for the simple reaction-time (not yet described). These developments came from conviction that the possibilities in the reaction-time field are relatively unexplored, especially in reference to the bearing of this field on the new conceptions of the response psychology. The fact that we have little factual data—not even knowledge of the effects of intensity of stimulus—seemed a challenge, and the scattered and uncertain nature of the past work appeared obviously due to the inability of any experimenter to settle down to a two- or three-year research on a single point. Further investigation convinced me that the inadequacy of apparatus was in part responsible for this situation. My own critical work on the Hipp led to the further revelation of awkward facts, such as the discovering the "standard" way of using the Hipp, reversing the current after each reaction, introduced the maximum of variable error, easily overcome by newer techniques. The discovery that the Hipp operated best without springs, and chance use of a synchronous motor designed by Lorenz led to the first model of the synchronous-motor chronoscope. The accessory apparatus for simple reaction timing has offered the greatest difficulty of all.

I have since applied synchronous-motor drive to other apparatus with success, particularly to kymographs, and, with the present availability of synchronous motors of greater power, a rapid extension of their use in psychological laboratories is possible.

My steadiness-test apparatus, overcoming the faults of the older simple plate, is a great improvement, and the double tapping-plate, eliminating the "tremor" and other "pseudo practice" effects is really a success. Definite research in correlating tapping with other performances is now greatly facilitated.

The "Felix" apparatus is still in the experimental development stage, but results are more and more encouraging.

My new pseudoscope, utilizing specially designed prisms, has not yet been utilized in research, and a number of other pieces of apparatus have their habitat still restricted to my own laboratories.

Recent new developments include apparatus for recording speech, and apparatus for work with both "active" and "passive" rhythm. All in all, I am immensely impressed with the present opportunities in American psychology to apply better apparatus to the more energetic attack on the wealth of experimental problems which surround us, offering thrills beside which theoretical endeavors pale into insignificance.

GIULIO CESARE FERRARI*

Professor Murchison's idea of tracing the trend of modern psychology by means of the autobiographical accounts of professional psychologists is such an ingenious one that it should prove to be extremely successful. Its success will be of great advantage to psychology if the psychologists who contribute to the series succeed in giving a careful and complete non-Freudian analysis of the psychogenesis of their ideas.

I say this because of my own experience, for the invitation to recall a past of which I was not in the habit of thinking afforded me the opportunity to observe that my intellect and my practical life have been upheld by a constant personal law which I had not in the least suspected. Indeed, all my life has been dominated by the need to study my kind in order to know it better. Since I am an active man, the past interests me only in so far as it touches upon the present or the future; I have therefore had to exert some effort in order to bring it to life. I used to think that I had an excellent memory, for my recall of past events was always accompanied by exceptionally clear sensory images. However, Dr. Murchison's request disillusioned me for I was forced to realize that my memory was very lacunous,[1] that I could recall only those of my childhood memories that were concerned with a psychoanalytic behavior of a certain type of which I shall speak later.

I am descended from an old country family of Vallona, a happy valley of Reggio Emilia. My father was forty-two years old when I was born and I therefore know little of my predecessors. The only one of whom I heard, always somewhat mysteriously, was a certain "Oncle François," an eminent ecclesiastic of whom it was said that he would one day become a cardinal. I, however, learned these positive facts: that he was red-headed and that he himself had undertaken the education of my father whose intelligence he had recognized. He had placed him in a Jesuit school for he intended to make a priest of him like himself. My father, on the contrary, had taken part in the conspiracy of 1848-1849 for the "Risorgimento" of Italy. And, as the movement at that time was against the Church, the mysterious "Oncle François" left my father to his fate. But, although my father had to support his mother and two sisters, he succeeded in becoming a barrister and a notary.

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*Submitted in French and translated for the Clark University Press by Gladys Kroll and Alda Hunter.

[1] A few examples will suffice to show the clearness of this fact: Of my eight brothers and sisters I have only twenty distinct memory images of my brothers during the time we lived in the country—up to my twelfth year. I have only six memory images (two of which are concerned with religious discussions) of my mother with whom I lived until her last illness which occurred while I was a medical student at the university. Of my father I can recall everything, even to the clothes he wore on various occasions. Oh, the mysteries of affective memory!

When I was born my father was Secretary General of the Reggio Emilia Community, and, while he always remained in the background, he was the virtual leader of the political administration of our small village. He was held in such high esteem by everyone that I was extremely proud of him.

My mother's father was in the magistracy, but I do not know in what capacity. Her family enjoyed a certain renown because of its intelligence and religious devotion. I recall how one of my aunts did not consider the Latin of the Church appropriate, so, in the *Ave Maria,* instead of saying *"fructus ventris tui,"* she used to say *"fructus mentris tui,"* obliging us to say the same. I often accompanied my father on visits to these aunts, whom he always defended against my childish criticisms, for I did not know how to reconcile the lauded intelligence of these aunts with their mental limitations, of which I retain the most striking memories. This impression must be a very old one for I cannot recall a single moment of sympathy for the aunts, although they always attempted to be very good to us. My father must have been a marked contrast to them for, while he was religious, even to the point of observing the practices, and was very much attached to my mother, withal he never spoke of religious things. If I asked him about religious matters, he would avoid the question without seeming to do so and speak on the splendors of the universe and the stars and of our intelligence with the Kantian arguments. He never spoke to me about religious practice.

He had trained me to examine my conscience every day before the evening meal, in a moral, practical, or social, rather than in a religious sense. To carry out this plan I would take a walk every evening at sunset in the most solitary parts of the countryside which I peopled with the heroes of all adventures.

At the time of which I speak my father had assumed the support of his two sisters, one of whom was a widow with two children, and the other had just lost her only child. Perhaps because of these two tragedies which had touched us so closely, I acquired the habit of making up analogous dramas for the families whom I knew, dramas in which there were sudden and tragic deaths, impossible marriages, and of arranging in one way or another the lives of the survivors. I enjoyed this play very much.

But once—I can still recall all the particulars of the occasion—as the fantasy went on a frightful scene was unrolled before my surprised eyes while a superior "I" whose presence I realized for the first time judged the scene, its horror and my emotion. My trend of thought had led me to imagine that my father had just died. It caused me little suffering for I realized that it was only make-believe. It immediately, however, gave rise to the idea, "In case my father dies no one will correct my Latin." No sooner had that idea occurred to me than I was ashamed that I could have such a mean and selfish thought on such an occasion. But the thought persisted despite all my efforts, "In my last theme my father had left some mistakes in Latin!" Perhaps I was becoming inured to the idea of the death of the person I most cherished. But while I was thinking of the fact and of my ingratitude I suddenly had the impression that I had discovered a force or a being within me who thought for me, who was stronger than I.

I had just come face to face with the confused image of my unconscious. That clear, exact, impression perplexed me. It was much later, while reading Hartmann, that I recognized that unknown of my childhood days!

I calculate that I could not have been more than six or seven at the most.

My games apparently all led me to psychological analysis, and, if I did not fear approaching the ridiculous, I would even say that a certain event was a psychological experience, although perhaps it was only the mischevious blundering of a child brought up a bit strangely by an exceptional father. A hundred particulars of the incident have convinced me, however, of its reality as a personal experience at any rate, and one which comes back to me every time I have some experience with a stranger, as if to inspire me with prudence. This incident occurred a few days after the birth of my youngest sister—I was therefore exactly five years old.

During the summer vacations the rich and noble family of S., with whom we were closely connected, used to come to live very near us. The head of the family was a cultured man but so haughty that he gave the impression of being proud without limit. He always dressed in black, wore a high hat, and walked so stiffly that it was said that he must have swallowed his cane. He always walked with head up, eyes straight ahead, with an absent air. He had been nicknamed the "Duke" (in memory of the Duke of Modena, I believe, who is still very much hated) in order to caricature the pride which he was thought to have and because he was thought rather hard-hearted. On the contrary, at heart he must have been very kind. I was his godson, but I could not bear him because he kept even his own children at a distance, which was in marked contrast to my father.

The occasion of the famous incident was on a cheerless and humid evening towards the end of October. Monsieur S. came to our villa to congratulate my mother who had just given birth to my sister. He entered the house without removing his hat. Now, my father always required us to be very polite in the family; we had to greet everyone on entering or leaving a room, allow women, even though they were servants, to precede us, and were never allowed to wear our hats in the house. While inculcating some rule of politeness, father always took care to explain it on a moral or hygienic basis, respect for the weak, the protection from the sun and rain afforded by the hat, etc. Perhaps the Duke, on entering, had excused himself for keeping his hat on, but I had heard nothing of it. My mother was on the second floor so that the whole family followed our guest up the stairs. I was consumed by the desire to inform Monsieur of his lack of etiquette, but I did not dare. The procession approached my mother's door; the moment had arrived. I approached the "Duke" and with a very bold air (my older sister still recalls the memorable scene), I said to him, "Monsieur César, it does not rain in houses." As long as I live I shall remember the surprised air, at first more of confusion, later of consternation, that showed on the faces of the family. But I was much more surprised to see the famous "Duke" look at me with his inexpressive air which became almost smiling as he said, "Quite right, little one." Now, I had already anticipated the disasters which might follow my offence to this local god—for example, the house might fall on me, or the earth might open to swallow us up. I was so sincerely humiliated that I no longer know whether it was due to the kindness of the "Duke" or to the miscarriage of the lesson which I had sought to inflict upon him, but I burst into tears, and I can recall nothing more of what followed.

The incident became known and, as Monsieur S. was generally disliked, I enjoyed, although secretly, somewhat of David's glory after his defeat of Goliath. On my conscience, however, was the humiliating fact that I had wronged someone who had perhaps not merited it.

This incident taught me an immanent lession of modesty and compassion which has remained with me always. It taught me to respect each individuality with which I came in contact for fear of hurting some individual's inner modesty.

My conversations with my father, whom I always accompanied from the village to the house, had taught me to observe everything and to seek to find for myself the reasons for the simplest facts. I still remember the day we went walking accompanied by the "Duke's" son and my father asked us about the alternation of movements of the arms and legs of two small soldiers who passed beside us and walked on ahead. We could think of no answer, and he pointed out, with excited wonder on our part, the gait of four-footed animals. From that day the examples which he drew, perhaps from his reading of St. Hilary and the two Darwins, constituted for a long time all the literary or other topics of our conversation. I enjoyed those conversations with my father immensely.

I believe he had a rather well-fixed idea on education, for he changed the topic of discussion every evening, sometimes despite my pleading that we continue the interrupted topic of the night before.

I think that he must have preferred my companionship for I noticed that I was the only one he ever spoke to about the town politics. This preference filled me with a little pride, although I remember that I was not at all amused. There were, however, some pictures which accompanied these observations and which impressed me very vividly and remain in my memory. For example: Men are very different in their physiognomy but even more so in their souls. But the passions are common to all and make all alike in the particulars of their behavior, as one soon learns to detect with a little practice. Passions are a sort of "sleeve" by which one can take hold of people in order to lead them in the direction one wishes them to go for their own good.

I no longer recall how old I was when I first heard this Machiavellian theory expounded, but I believe it was before my twelfth or thirteenth year, for at this period I read, with some friends much more cultured than I, *The Tales* of Edgar Poe, especially his famous "hidden letter" (which the author had left exposed to all in order to lead astray any clever person who was looking for it). A short time after we read certain of Paul Bourget's *Essays* which opened entirely new horizons to me on the real simplicity of the human soul. Now, these readings only gave a meaning to, and later a certain attraction to, certain of my father's talks which had at first not interested me in the least.

Some years later, with the same friends, I read Stendhal's *Le Rouge et le Noir* and the *Chartreuse de Parme* which determined, not only in a literay way, the trend of my intellectual preferences. I could do no better than to try to follow the course indicated in order to know men and women as thoroughly as my father knew his fellow citizens.

Chance offered me an opportunity, among many others, for an attempt which was highly successful. I knew a boy of about fourteen, intelligent but unwilling to do anything for he was wealthy and the child of an ill-sorted couple. He was attached to me because I spoke to him as if he were older, pointing out to him the attraction of biology in which I was very much interested then. I set myself the task of turning him into a respectable young man by creating interests in him that contrasted greatly with the tendencies of his environment. The only way to keep him on a high plane was to force him to study new things. I persuaded him to become a socialist. He was very much interested in the subject, but it was the economic side of it that attracted him rather than the sentimental side. In any case, he was saved. He became a philosopher and was greatly appreciated by young people. Unfortunately, he died at the age of thirty-five: he was Mario Calderoni.

This brief account indicates that my passion for analyzing the minds of people was very precocious. From my observation of the psychological development of my first two children I may say that this passion might even be congenital. I refer now to the conclusions of a study I made when my children were twenty years old. This study is based on concise and absolutely objective notes taken daily by my wife during the first two years of the children's lives.[2] Here is the conclusion: "My observations show that the line of demarcation between what will later by aptitudes and moral character of young people makes its first appearance toward the end of the third month and becomes determined and perhaps fixed during the sixth month."

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[2] Lo sviluppo intellettuale dei bambini "avanti lettera." *Riv. di psicol.*, 1925, **21**, 171-190.

Naturally, in my own case self-analysis scarcely permits me to go as far back as the third or the sixth month of my life, but it is quite possible that from the third or fourth years this inclination of my personality penetrated my consciousness and remained there.

I have a curious impression of my eight years of secondary schooling, an impression of never having studied and of having learned nothing there. Since I never repeated any of my school work, the inference that I did learn must be drawn, but I can only recall the roguery of pupils worse than I.

In these secondary schools I acquired a habit of which I still retain the advantage and which I utilize to the present day. I had found an outlet in very vivacious literary criticism for my passion of seeing various traits beneath a person's habitual appearance. I was greatly interested in reading two papers with opposed views on all subjects. I had to read them during school hours for the newspaper vendor loaned them to me for the mornings only. In order to do this I folded the papers horizontally in folds from 30 to 35 centimeters wide and kept the paper under the desk so that I could read six or seven columns at once. I had to wait for a moment when the instructor's attention was elsewhere and then quickly turn the paper in such a way that I could see the successive segments of the six or seven columns I had started. From this practice of subdivided memory, continued for some years, I have acquired an ability to be interrupted at any instant in no matter what work of reading, writing, or observation and to be sure—mathematically sure—of resuming the reading, writing, or observation at the exact place where I had left it. I believe this practice is also responsible for the speed in the shift of my attention, which makes my conversation difficult to follow, my friends say, and of which I had a good example in my public examination for admission to the professorship of psychology at the University of Bologna. At a certain moment, while I was expounding my doctrine, I pulled my handkerchief from my pocket and tied a knot in it. Evidently I was interested in recalling afterwards some vague association aroused by some remark I was making—a double train of thought!

When I was twenty-three I noticed my inability to remember numbers. For me they are simple patterns. Of the multiplication table itself I only remember the combinations that have a marked assonance. I have a great deal of trouble, also, for example, in remembering a telephone number even for the time necessary to repeat it over the instrument. I have illustrated this extraordinary anomaly which extends to anything mathematical in a short descriptive study wherein the phenomenon is explained as an autosuggestive act of an affective character.[3]

I studied my medical courses very carefully, although I had chosen medicine, I believe, in accordance with a common observation of that materialistic time "*que les medecins avaient substitués les prêtres, pour la maîtrise des consciences.*" I took no special courses, not even psychiatry.

At the University I had allied myself somewhat with Guillaume Ferrero who wanted to develop in me a taste for Kant. Ferrero's influence was so great that I made every effort, but I could not succeed in acquiring the taste for philosophical thought.

I had an almost physical need for feeling something solid under me. All my life I have suffered from this inability to transfer my admiration for certain philosophers (Spinoza, Schopenhauer) to a taste for abstract thinking of philosophy.

The need to envisage the problem of will in positive terms arose imperiously in me when my dearest friend (he who had introduced me to Poe, Stendhal, Bourget, etc.) committed suicide. The problems of the will—of free will, of moral responsibility—were scattered before me like an impalpable cloud. I made a sort of psychoanalysis of my friend, completing it by a study of his heredity, his constitution, his abuse of laudanum and tobacco. I came to consider my friend's fate as a clinical case and from it made my first study on "the diseases of the instinct of preservation"[4] of which I differentiated between two forms, the "*indifferentia vitae*" and the "*taedium vitae.*"

I became a doctor in 1892 and accepted a position as assistant in the most famous insane asylum in Italy, the Psychiatric Institute of Reggio Emilia, under the scientific direction of Professor A. Tamburini. There were, for the more than one thousand patients, ten doctors, among whom were Vassale in pathology, Guiffrida Ruggeri in anthropology, and Guicciardi in psychology. There was also an enormous library.

I knew nothing of psychiatry, but I did not dare ask for direction or training for I was afraid of showing how ignorant I was on the subject. I began the task, alone, of observing the patients and checking the clinical notes with different descriptions which I found in various authors, among whom I preferred Griesinger.

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[3] Un caso di amnesia parziale continua. *Riv. sper. di fren.*, 1895, **20**, 509.
[4] *Il pensiero ital.*, 1892, No. 23, p. 26.

My first clinical work on the writings of certain patients testifies to my intellectual solitude at this time.[5]

I traveled a little; I went to Turin where I developed a great affection for Cesare Lombroso, a man of great genius and a great disseminator of ideas, whose criminal anthropology was beginning at last to gain favor in scientific circles; and to Florence where I met another man of genius, already on the decline, Paolo Mantegazza.

I returned to my position in the asylum of Reggio under the same conditions that prevailed before I left. Professor Tamburini had appointed me editor-in-chief of the *Rivista Sperimentale di Freniatria,* which at that time was the only review in that field, for he well knew my wide interests in all the fields of psychiatry.

This proof of Professor Tamburini's confidence made my work at the asylum of Reggio harder, for I was fascinated by psychology because of all I had read of it in fiction, by all I had gathered in going through the articles in the journals in that field, all the at-that-time-modern publications—Bain, Spencer, Wundt, Baillarger, Charcot, Féré, Binet, Janet, Richet: my mind was taken by this will o' the wisp which must have existed but which I did not know how to reach—the scientific study of man.

In a window I saw a Hipp chronoscope which Buccola had used in his famous study on "the law of time in the phenomena of thought" (Milan, Dumalaro, 1883). But, not knowing the instrument, which I dared not touch, I did not understand his book very well. Even if I had understood it, however, I would have recognized with difficulty the elements of that mysterious thing which was filling me with such enthusiasm. The psychology of Wundt and of many others affected me in the same way.

My timidity, which was accentuated by my position as editor of the celebrated *Rivista di Freniatria,* placed me in such a position in relation to the other physicians at the asylum that it was hard for me to ask the help I needed. Moreover, no one suspected my need, especially since all my colleagues knew how busy I always was reading books on normal and pathological psychology and because of my great interest occasionally even in articles on psychical researches.

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[5] La degenerazione dello stile nei paranoici erotici. *Riv. sper. di fren.,* 1894, **19**, 329.

The appearance of the first volume of Binet's *l'Année psychologique* was one of the most important events in my life. On going over the index of the bibliographical abstracts, I found several articles correctly listed which I had never known how to classify. Truly my feeling was one of physical relief. The reading of this index was a magical "Open, Sesame" for my intelligence.

The state of relief and gratitude which this index brought me was strengthened and increased when I read Binet's masterly preface. It brought order out of the confusion that had reigned before in my head in regard to my psychological knowledge. A great commotion occurred in my mind that day that was calmed only when I had written an expression of my gratitude to M. Binet. It was difficult for him to appreciate to what extent he had brought logic into the mixture of confused and unorganized ideas which I had stored away in my mind.

I slowly learned to read, now that everything I read fell into its proper place. I still can feel the satisfaction which came to me on reading Alfred Binet's studies on the dramatists, for these studies, in the first place, were linked to my own infantile fantasies and, secondly, to my attempts when I was a youth to direct other people. I had been using Alfred Binet's psychology (as I have related before) without knowing it!

With my father's aid, I obtained a scholarship and hastened to Paris where, in Binet's little laboratory at the Sorbonne, I worked as much as I could with Binet, Courtier, Jean Philippe, and Nicolas Vaschide. With Binet, who honored me with his friendship and introduced me to his charming family, I studied the question of tests which Cattell had just made popular after the exposition at Chicago and which Binet was to launch into the world under his famous title of "mésure métrique de l'intelligence."

The death of my father obliged me to return to Italy, and I resumed my former position at Reggio. There Professor Tamburini at once made it possible for me to found what was to be the first psychological laboratory in Italy. I had now much more self-confidence than before and two of the physicians at the asylum, Guicciardi and Bernardini, wished to be trained in psychological work. I kept for my own research special tests in individual psychology, while I worked with Guicciardi in verbal associations and the psychology of calculators and of thought readers and with Bernardini on musical memory in idiots. Some young students also worked with me, M. Calderoni, G. Ruffini, and others. I published a great number of articles both by myself and in collaboration. Among these I am particularly fond of certain ergographic researches made on women.[6] At that time, with the exception of Mme. Klumpke-Déjérine in Paris and Mlles. Joteyko and Kipiani, it was hardly possible anywhere in Europe to find women in laboratories or to have them as subjects. However, in Reggio I was the physician in the women's section which adjoined the psychological laboratory. Therefore I had women by preference as my subjects, both patients and nurses.

Using the Mosso ergograph, I noticed at once that a woman, even though she may be right-handed in ordinary life and in her use of the dynamometer, usually gives an ergographic curve for her left hand which is infinitely superior to that for her right hand in respect to the number of flexions though not to their height. The curves for the right hand are analogous to those for men. In brief, women habitually show a static resistance in the right arm which is exceptional.

I explained this phenomenon by the fact that primordial woman had to work with her right hand while she held her baby on her left arm. This took place during a period of years, for procreation was not limited at that time nor could the babies be handed over to nurses.

My good friend, Giovanni Papini, who was later to become a famous philosopher and artist admired by all the world, agreed with my explanation. He further observed that perhaps it was for this same reason that painters and sculptors of all races have depicted women through the ages as buttoning their garments on the left shoulder, a habit which is exactly contrary to that of men. We still have traces of that century-old difference in our own clothes. This feminine habit of buttoning their garments on the left shoulder permitted them to nurse their young more easily while they were working.

My own personal interest lay in researches dealing with individual psychology applicable to the insane. I spent much time in this field and finally prepared two groups of tests. The first one involved no apparatus and was made up of very simple, practical, and well-chosen tests which could be applied in all asylums, prisons, etc. The second group of tests, which was much more complex and needed to be completed by all the help that a physiology or psychology laboratory could furnish, was reserved for psychiatric clinics where more complex psychological research could be made of use. It was a complement to the first group of tests.[7]

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[6] Ricerche ergografiche nella donna. *Riv. sper. di fren.*, 1901, **24**, 61.

It is easy to see that if the suggestion of this work had been adopted, the principle of the unity of stimulation, joined to the simplicity which permitted a very easy repetition of identical tests, would have been a considerable aid in the examination of the insane and would have increased greatly our knowledge of the evolution of mental diseases. The memoir dealing with these matters was crowned by the Turin Academy of Medicine, but the idea did not have the response that is should have had.

In 1898, during a reception at the home of the distinguished philosopher Mariller at which Janet and others were present, I had occasion by chance to read some pages of William James's *Principles of Psychology.* I was so amazed by the vivacity of the book that I planned to purchase it immediately. The personality of the author, so profound and at the same time so brilliant, strongly appealed to me. I was much surprised that a vividly written book of such scientific vastness and literary beauty was not better known in Europe.

I greatly desired to translate it into Italian. Enthusiastic as I was, I thought that such a marvelous book would miraculously awaken us to an interest in scientific psychology. Moreover, in my opinion it was necessary to orient rather than to nourish the exuberant youth of Prezzolini, Papini, Gallette, Vailati, Calderoni, and all the pre-war generation which had found no outlet other than literary or philosophical criticism.

I found De Marsico, the Director of the Societá Editrice Librariá at Milan, to be an enthusiastic publisher who understood and approved of my idea, for as soon as I had come in contact with the noble intellect and ardent spirit of William James which enkindled such admiration, I went to him and explained my reasons for publishing the book. De Marsico helped me in every possible way and I began at once my translation of the *Principles of Psychology.*

The translation of William James was an enormous success. Four editions rapidly followed one another, and I am proud that this translation served in the spiritual elevation of a number of young people who had been blindly searching their way after the failure of positivism.

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[7] Metodi pratici per le ricerche psicologiche individuali da adottarri nei manicomi e velle cliniche. *Riv. sper. di fren.*, 1903, **26**, 788.

During these years when psychology was so popular in Italy, thanks to the success of William James it was said, Professor Bianchi, while Minister of Education, founded three chairs of experimental psychology at the Universities of Rome, Naples, and Turin. But the professors who upheld pure philosophy objected to such an extent that even today all other universities must content themselves by appointing "Professori Incaricate" (the French chargés des cours) as teachers of psychology. Examples are Bologna, Padua, and Florence.

In my laboratory I was busy with small experimental studies in musical expression, but I was handicapped by my need of a performer. I was also interested in the psychological examination of the blind,[8] and I brought out the facts of the particularity and the total inferiority of thinking in the blind due to their lack of a means of gaining information which is so important.

At the same time I was engaged in work on the insane. From the psychological point of view I published one of the first demonstrations of the prevalence of the affective elements in the genesis of all mental disorders.[9]

During this time my interest in the insane gave me an opportunity to work towards their amelioration, i.e., professional instruction for the attendants, family assistance, legislation, etc. I traveled a great deal in northern Europe where I had the opportunity to see and admire what Decroly and Claparède were doing for the mentally retarded and for normal children.

I became so interested that in 1903, after eleven months of directing one of the asylums in Venice, I was given the charge of a private institution for more than three hundred serious cases of mental retardation at Bertalia near Bologna. I shall speak of that later.

It was an auspicious moment, for interest in the insane was in its ascendancy. With my friends, Vailati, Papini, and Calderoni supporting me, I founded my *Revista di Psicologia,* which began as a review of psychology as applied to pedagogy and psychopathology and was later to become a journal for general psychology.

This journal, which existed solely because of my desire to create sympathy for psychology and which was entirely supported by its subscribers, naturally was governed by the varying winds of the time and by the sympathies of amateurs in psychology and pedagogy. Therefore, from it one can get a vivid picture of the two movements of psychology and pedagogy, particularly in Italy, during the last twenty-five years. This wavering line can well be understood through running over the list of original articles.

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[8] L'esame sperimentale dei ciechi. *Riv. di biol.,* 1905, **3**.

[9] Gli stati emotivi nella genesi delle ossessioni e dei deliri sistematizzati. *Riv. sper. di fren.,* 1902, **27**, 656, 661. I resumed the argument in: Psicologia et psicopatologia. *Riv. di psicol.,* 1924, **20**, 1.

In 1906 I was put in charge of the course in experimental psychology at the university school at Bologna with the object of improving the school instructors. I occupied this position up to the War.

In 1907 I obtained the "libera docenza" in experimental psychology in the Faculty of Philosophy at Bologna (I was already "libero docente" in psychiatry since 1903), and the following year I was put in charge of the same instruction for philosophy students, an appointment that has been regularly renewed. My courses have always been well attended, and my students received their training in the psychological laboratory which I had established in the Bologna asylums that I directed.

As a result of a competition in 1907 I obtained a position as director of the famous asylum at Imola, near Bologna. This asylum, very beautiful architecturally, was renowned both because of the loveliness of its gardens and the number of its patients (550). However, my predecessor had not taken much interest in his work, and, as a result, the asylum was like a beautiful woman who is bedecked and heavily painted: one never knows just what may be underneath.

I had written so much on the bad condition of asylums of the day and on the amelioration that was so urgently needed that I was obliged to do all I possibly could to make my own asylum a model one.

In 1904 or 1905 I was especially interested in an institute for mentally retarded cases which I was in charge of, and the insane had rather taken second place in my mind. I was about to translate William James's *Varieties of Religious Experience* when I received a very voluminous manuscript from him accompanied by a letter which said in part: "This manuscript has been sent to me by a young man, who, so he says, has just gotten out of an insane asylum. Read it and tell me if it is possible that what he says is true. If it is true, a civilized man could have no better task than to devote himself to the work which this man begs."

The book in question was by Clifford Whittingham Beers. Published in 1908, it became very famous, exerting a moral influence, it is said, comparable to that of *Uncle Tom's Cabin* on slavery.

My response to William James was that, although the author had pictured the asylums from a too personal point of view, still he had probably told only the truth. Several months later, when I was called to be the head of our asylum, I thought I had forgotten the little incident, but when I received a copy of the published book of Mr. Beers, I realized that, as director of an asylum, I had only to follow the teachings that a conscientious man, a physician of an asylum, could and should derive from this famous book.

Naturally, Mr. Beers, in recounting his experiences as an insane person, had succeeded in arousing public opinion against the incompetency and ill-will of asylum attendants in general and of certain physicians. However, the tale of his useless sufferings unrolled in a still more angonizing fashion before the eyes of those who, knowing the life in asylums, realized that, more serious even than the mistreatment of the patients, was yet another condition for which neither the patients nor the doctors had found any solution. Inevitably, for a thousand reasons that I cannot explain here, an insane person is considered as debased, as an inferior being. He may know clearly that he is not such a being, but he knows that he is doomed to be considered such. Even when he is dismissed from the asylum, he will likely never escape from this feeling which is the worst in the world for patients who are lucid and conscious of their condition.

I have always understood this fact and acted accordingly. I have oriented the treatment of my patients on an absolute respect for their dignity, striving to reconstruct it whenever it had disappeared or been wounded.

The future will undoubtedly prove that this was a proper course to take. If I have been a pioneer in doing this long before the important movement for mental hygiene was initiated in America, I owe it to the influence of Beer's book, an influence which I was only to be consciously aware of four or five years after I had received the first impression.

My best and most successful practical application of psychology has been in connection with children who have fallen into the hands of Justice—with a capital J—and who are consequently called criminal children. Just as my passion for all aspects of psychology showed itself in my earliest childhood, so arose very early in my life a doubt as to the reasons and imputability for wrong doing.

The following experiences aroused a skepticism in me which is now extended to all court acts. Please bear with me while I relate two little incidents which were very important in my life.

I had been taught that stealing was a great sin, quite complicated, and was a mortal sin even though the thing stolen was without value. I was probably about five years old when a family of our acquaintance came to visit us. They had a small boy of my age (now a general in the army) who had brought with him a magnificent, shining toy of polished copper, decorated with a splendid red varnish. It was a small fire-engine, and we could spurt water a meter away when it was filled. We had been playing with it several hours when my attention was caught by a little rubber tube, ten to twelve centimeters long, which was a part of the engine.

The next time that I went into the laboratory to replenish the water, I jerked off the little tube. The water then flowed very feebly. Not for a moment, however, did I think to confess my fault. But the little tube in itself no longer had any attraction for me, and I tossed it on the top of a large wardrobe. I gave the toy back to my friend, although it now was useless because of my naughtiness, but neither he nor my brothers said a word about it. We played with other things, and that was the end of it.

Several days later, in spite of my unwillingness, I had to go to my friend's house for luncheon. There on the mantle of the dining-room shone the famous engine, quite new but quite useless. As there were many mirrors in the room, I could see nothing but the engine, no matter where I tried to look. But no one mentioned the matter. Why? My small head was bewildered in its attempt to solve a twofold mystery.

To what incentive had I yielded? Not to covetousness, for as soon as I had stolen the little tube, I threw it away, and I was at great pains not to go near the wardrobe which was near the scene of my guilt. Not from an act of unfriendliness, for I was very fond of the little boy and was not at all jealous of him at that time. Why did I do it, then?

The second problem was no less troubling. I was absolutely certain that the friend, whose toy I had destroyed, and his family knew that I was guilty. And nobody said a word about it. Why?

Anyway, since I had stolen, I was a thief. And, in spite of this sin which was so vile, so evident, so well known, nobody seemed disturbed about it. What was God doing? What were people about?

My emotional disturbance was calmed only when I found courage to confess my guilt to my father. He was able to console me for the theft, but relatively he complicated my confusion as to the reasons for the silence of my friend's family, for he used an expression which further troubled me, "That happened because the M.'s are Jews."

It often happened that my father would give me only half an explanation of a question and several days later he would ask me if I had completed the reasoning.

I knew that Jews at this time were badly looked down upon. I did not know, however, that we were the only Catholic family at that time who accepted socially the M.'s, who were Jews, and this was done in spite of the extreme religiousness of my mother. My father's phrase was meant to convey to me that this family had probably acted in this manner because of the gratitude which they, as Jews, owed to our family for placing them on our social level. But this single phrase infinitely increased the confusion in my ideas, for a more troubling element than ever was added: the question of the religion of the people whom I had offended.

My mother's family and my Catholic friends severely condemned our friendship with Jews, who, according to them, were the bane of humanity. Even if Jews were good men, they were doomed after death to remain in hell during all eternity. Now, by their silence these people had shown that they had forgotten or forgiven my heinous crime, a thing that I, a Catholic, had never done. And my father explained this fact by their religion! (I believe that my relative indifference to religious questions owes its origin to these troubled days of my childhood.)

In any case, what was the mystery involved whereby the religion of the person offended against should have affected the guilt of the offending person? I had stolen without reason or profit, but still I was a thief, and everybody could and must have known it. This mortal sin had no sanction. What was the meaning of it all?

A second experience, caused by my father, occurred when I was twelve or thirteen years old. I had been so deeply affected by reading a sentimental tale (*Furio* by De Amicis) that I wept for several hours. Afterwards, I was so displeased with myself and thought myself so stupid for being moved by a written story which could be invented that I was ashamed to confess the silly reason for my deep sadness.

I had hidden in the attic in order to cry at ease. My father, however, followed and tried to console me. Seeing that I was so very upset, he said, "I understand. You have stolen something." I denied having done so, but I continued to weep.

Several days later I confessed privately to my father the reason for my tears. He accepted my explanation, but I could see that he was not convinced of my sincerity. Since my father constituted for me the whole of universal conscience in that he knew everything and understood everything, my ideas on the positive, real foundation of honesty were thrown into confusion. I said to myself, "When I had really stolen, no one reproached me in anyway whatsoever, and now when I am utterly innocent, my father, who possesses greater authority for me than my own conscience, my own father, who knows that I have never been able to lie to him, believes that I have stolen."

My ideas on what was moral, in what one ought to do, and why one ought to do a thing or abstain from doing it, troubled me so profoundly that, as often as possible, I used to walk around the village prison, pitying the fate of the inmates whose companion I might be some day. Perhaps they were the victims of false accusations, even, as in my own case, on the part of those who loved them the most. I was very despondent over the fate of the world in this respect.

I am certain that this memory has contributed greatly to the enthusiasm with which I have accepted the possibility of gathering into my institution young criminals. Living with them, I intended to find out what crime was for a child and in a child.

Now I shall give the history of my very interesting test of that question. In 1907 I was given the directorship of the Imola asylum and at that time it contained about thirty mentally retarded children. I constructed a model section for them, bringing in as the director a young teacher, Professor Gabriella Francia, who was the most intelligent woman I have ever known, clever, reserved, and yet enthusiastic beneath her calm exterior.

Affairs progressed very satisfactorily, and I was more and more convinced that all mentally retarded cases should be carefully studied, for very often we found a case of mistaken abnormality among them. However, when one is certain that the patients are really mentally deficient, it is best to train them to do some kind of useful work without troubling to give them much school instruction.

In 1907 I gave a report at the Congress of Relief for the Insane at Amsterdam on the statistics of my experiences at Bertalia dealing with the records of children who had left the institution as having been helped. Nearly all the young girls had become prostitutes or were in the lowliest forms of domestic service, while only a few were still with their families, who were tired of supporting them.

One day I spoke about this serious problem to Count Rasponi who was then the president of the Bologna court. He was a very kindly man and, in spite of his modesty, was so intellectual that his sense of justice rose above the routine and shackles of the red tape of his office. A number of minors who had been caught in the toils of the law had to be confined with adults for varying lengths of time in the Bologna prisons. This condition troubled the kindly president, and he suggested that I take these minors into the section for children which I had instituted in my asylum.

Although this proposition offered enormous difficulties of all kinds, I accepted it without too much reflection, on the condition, however, that there should also be admitted those children, abandoned or not, who had come or were about to come into the clutches of the law. Evidently, I was increasing the difficulties of administration in my institution by this proposition, but I had a vague, general idea in mind which had been fostered by the concealed but powerful action of my subconsciousness. Furthermore, I could count on the intelligent and certain help of Mlle. Francia.

This subconscious idea, which had been germinating in my mind and pushing me into action, was that of reaching a similar treatment for both the insane and the criminal. I had often thought of the biological and social analogies that link the two groups, and now a pragmatic demonstration of my ideas presented itself as a very interesting proposition. Moreover, the natural procedure was to begin this demonstration with children.

Formerly, not much more than a century ago in fact, people believed or perhaps feared that the insane were victims of God. No civilized being believes that any longer. It would be very interesting to show, even in a very small way, with children, that a similar unreasonableness, due to ignorance, governs our ideas in respect to criminals. The latter are simply sick folk of a certain sort whom we habitually mistreat in a spirit which is entirely unworthy of our stage of civilization.

Everyone knows, at least all the physicians know, that there exists a mental disease called moral insanity. But everyone knows and admits that by far the majority of these sick folk are not judged and treated by physicians but by lawyers and judges, and the medicine used consists of years in prison, torment, and even capital punishment. And this is done, perhaps, because the actions of these morally insane persons menace the property and integrity of the "man in the street," and, even more, of the men who make the laws.

The possibility which was offered me by the president of the courts of Bologna meant all of this to me. Evidently, my imagination was at work, for at this time (1910) a certain movement in favor of minor delinquents had already been begun to a certain extent in special institutions for children, particularly in special courts for minors.

Enthralled by my ideal plan, I believed that I could help establish in a practical way a demonstration of the complete parallelism existing between the insane (as I had begun to understand them) and criminals. At the same time, the experiment would give me an opportunity to become better acquainted than ever with the psychology of delinquent children and the possibility of adapting them to practical life.

Having some money at my disposal, I rented a house set in a magnificent park, and here I gathered together about forty children of both sexes, nearly half of whom were mentally retarded cases from the asylum with clearly defined anthropological defects, the other half consisting of "morally insane" children already belonging to the asylum or of children whom Count Rasponi had collected from the prisons, foundling homes, or other Bologna institutions.

Mlle. Francia accepted with high spirits and a magnificent optimism the proposition of directing this colony, aided only by one nurse and four elderly patients from the asylum who, I thought, could do the heavier work of the establishment as well as teach the children themselves to do the field and house work (to make the bread and do the general baking, to keep everything clean, to carry water, to gather and prepare the vegetables, etc.). I thought it would be very useful to the children who returned to ordinary life to know how to work in the country and to be useful in poor surroundings.

The general principle which controlled the colony was that of confidence and sincerity. No distinctions in respect to study, food, or amusement were made between the children who were here living together, and only the dormitories and lavatories were separate for the two sexes.

The cardinal point of our system was treating all the children on the same plane. We had set certain regulations for their habits of living, but, after explaining these, we never reprimanded or preached to the children. If a child violated the rules governing the general good and usefulness of our community life, we explained that it was necessary for him to stay in bed until he became normal again, that is, until he should no longer do any harm to his companions or to the general management of the colony either by his example or in any other way. Going to bed and remaining there for several hours or even days soon came to be considered as a measure for proper law and order and not at all as a punishment. The children quickly understood the necessity for this rule and, soon, the usefulness of it. Accordingly, it immediately lost the character of being a punishment.

The delinquent children came themselves to understand in this way something that was never directly told them, that is, that they were abnormal in the same way as were the mentally retarded children, even if their illness spared their intelligence.

Mlle. Francia has compiled a very complete report of our experiment,[10] a report in which the psychological observations for each proposition are manifest.

All the children who took advantage of our colony were enormously benefited in that they were helped to establish a personality which neither added nor took away anything from their original individuality but which simply oriented towards useful ends their tendencies which were not good or were even decidedly bad, diverting along harmless channels the excess activity of their vitality.

The most marked improvement, however, has been the least noticed, the matter of their moral feelings. We devoted our attention to the child's unconsciousness which is always ready to receive impressions without the child's awareness. Thus, little by little, a new feeling was born in him, which, perhaps because of its novelty, astonished and attracted him. And if it happened that the environment was such that the child became interested in this state of experiencing a new moral sense, the advantage brought to bear on his conduct by this new feeling was unlimited, and its triumph would become permanent.

Coeducation in our colony (a very new thing in Italy in 1910) never gave us any trouble and was of great value, I think, in the success of our experiment. However, considering the nature of our patients and the previous records of the delinquents who came to us from Bologna, this was a very surprising fact.

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[10] Francia, G. Primo esperimento di colonizzazione libera dei deficienti gravi e dei giovani criminali. *Riv. di psicol.,* 1911, **7**, 1-46.

We received as members of our colony certain children, who were abnormal in intelligence and character, belonging to wealthy families. We received them with the single stipulation that they must live absolutely under the same conditions (beds, clothing, and food) as our poor children.

We had the opportunity to show our colony to several members of the International Congress of Philosophy which met in Bologna in the spring of 1911 and of which I was the general secretary under the presidency of Professor Fr. Enriques.

I am able to say that from the eighteen children sent to us by the courts as "criminals" (according to the code previous to the War) only two hysterical cases fell again into the toils of the law and one of these had won three silver medals in the War through acts of extraordinary valor. Two of the eighteen died gloriously, facing the enemy, and the rest of them of both sexes have lived very honest lives.

Living with these children as I did, I soon saw the great harm that was done them by the logical but terrible coupling of the two words, child and criminal. It is perhaps correct from a grammatical point of view to call criminal a child who commits crimes, but, if age determines the nature of a child's mind and the law determines what acts should be called criminal, it is quite evident that the same acts are absolutely different according to whether they are committed by an adult or a child.

I saw what enormous importance the environment in which children live had on the birth and development of quite normal tendencies, and I judged that it was probably the environment, policemen, muddled and blind judges, which gave rise to what was called criminal tendencies.

The good effects of my colony, the successful operation of which was interrupted by the War, and the fact that the same results were repeated every time children were put in equally good conditions, have clarified my ideas to such an extent that it is very easy for me to pick out from the large number of children brought under the power of the law the rare cases of boys who are really ill and who should be cared for and separated from the others.

I spend much of my time in making public these facts, the fruit of my twenty years' experience. It is not my fault if humanity progresses less rapidly than science.

As a result of my activity in favor of criminal children and because of the unusual character of my experiments, I had the honor to be called by the Minister of Justice Mortara to take part in a commission presided over by Enrico Ferri which was to write a new penal code based on the principles of criminal anthropology as pronounced by Lombroso and Ferri. In the code which we prepared the criterion of "moral responsibility," which governed all codes, was substituted for the criterion of "legal responsibility" according to which the law should be chiefly concerned in the defense of society through measures depending exclusively on objective factors and not on philosophical or moral principles.

I brought to the commission, in collaboration with De Sanctis, the help which we could give as a result of our long experience, especially in regard to problems concerning the insane and minors.

The changing regime in Italy (1922) put our work to one side. Happily, the Ferri code (for, according to usage, it bears the name of the president of the commission, Enrico Ferri), a stillborn infant in Italy, has found more favorable conditions among the Russians of today.

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All the important happenings which might possibly have any psychological bearing have attracted my attention, ranging from terrible cataclysms, such as the earthquake which destroyed in a few minutes the Sicilian village of Messina,[11] up to the War.

I shall pause for a moment on the psychological question of the Elberfeld calculating horses.[12] Shortly before the War these famous horses had amazed the psychologists of Europe. Faithful to Myers' principle, I had gone to see them with De Sanctis. The spectacle was cleverly arranged, but I received an impression, at least, of "bluff." However, the bluff was so well gotten up at the famous Elberfeld equestrian school that I needed to clear up the matter in my own mind.

I took advantage of two facts: one of my friends had an Arabian horse (like those at Elberfeld) which seemed to be very intelligent, and I had a little three-year-old daughter who as yet had no idea of arithmetic. I undertook to educate both my friend's horse and my small daughter strictly in accordance with the methods used by the jeweler Krall of Elberfeld who had discovered the mathematical accomplishments of his horses. The result was that the horse surpassed my daughter in rapidity of learning and counting up to five, but after that he stopped in a clear-cut manner, while my daughter continued to progress steadily.[13] Naturally, I limited myself in this experiment, not going into the higher calculations which the German metaphysicians delight in.

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[11] La psicologia degli scampati del terremoto di Messina. *Rev. di psicol.*, 1909, **5**.

[12] La "scuola dei cavalli" à Elberfeld. *Riv. di psicol.*, 1912, 8, No. 5. Che coza pensano i cani che parlano? *Riv. di psicol.*, 1920, **16**, No. 1.

And then came the War. I was not able to live without observing at close quarters this extraordinary event.

My university situation and a publication on the effect of combat on wounded men who had returned from the War won me permission to wander freely about the front and through territory which was in a state of war. I gathered from these experiences many interesting observations, particularly with regard to the oscillations of courage which take place during war in general and during actual combat. I also gathered an enormous quantity of observations on the morale of the soldier and on the rôle played by the subconscious in courage as well as in fear. I was able to follow the effects of the defeat at Caporetto, and I had the opportunity of observing at first hand the psychological mechanism of the miracle of the rebirth of conscience and of the will to conquer in the soldiers as well as in the people, the thing which led us to victory though our forces were almost without allies.

I published my observations, which had been regarded as entirely correct by several very competent men, in my *Rivista di Psicologia* in 1915-1916, where I also published Mussolini's "Diario di Guerra" in 1916. He was a simple soldier in 1916, but he was to be revealed later as a man of genius. In my introduction to his lucid account I hailed the moral and representative worth of the man. What followed has shown that I had judged rightly.

In 1922 I also published an article on the "psychology of the Fascist revolution," which possesses only an explanatory value of a fact which has been of great value for Italy, for in 1922 we were in the midst of a Latin bolshevism.

The War over, I became interested, together with Claparède, Mira, Christiaens, and others, in questions of psychotechnics and the scientific organization of work in general. I took part in international conferences on the subject, and I successfully organized the second meeting at Milan in 1921.

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[13] Ferrari, G. C., & Pullé, Fr. Il primo mese di istruzioni d'un cavallo. *Riv. di psicol.,* 1913, 9, 176.

With the help of my son, an engineer, who is very adroit in everything mechanical and endowed with a very fine scientific mind, I performed certain special but rather unrelated researches. I am convinced that time and movement studies are very valuable aids in the economical regulation of factory work, for they are of service (as are also mathematical calculations) in organizing with the greatest possible precision the complexity of work from a suggestive, psychological point of view and in determining the most profitable procedure both for industry and for the worker.

I am still engaged in determining the importance, which I believe to be quite considerable, of the unconscious or subconscious elements in industrial fatigue.

I am also interested, though unfortunately without too much success in Italy, in the questions of mental hygiene. I wish to mention this *penchant* of modern societies because the World Congress of Mental Hygiene, which was held in Washington in the spring of 1930, gave me the wonderful opportunity of visiting certain places in the United States of America, a part of the world which in many respects is the vanguard of modern civilization, a country to which I owe my acquaintance with William James who has shaped my mind as a psychologist, a country from which I have drawn my ideas of the technique of individual examination of the insane, and which has given me in the book of Clifford W. Beers a description of the soul of the insane which I think is quite novel and genuine.

* * * *

It would not be easy for anyone to answer the concluding questions in Professor Murchison's letter. It is even less easy for me, as I am naturally timid and am afflicted with an intellectual modesty which is very often embarrassing.

In the first place, I belong to that category of persons who do at first what is suggested to them by an inner spirit or by circumstances and who proceed from these performances to relatively general ideas. My long experience in this has caused me to have great confidence in the work of my unconscious.

I believe that I have been useful to scientific psychology largely through arousing interest in that science and through spreading information concerning it by means of my laboratory, my translation of James, my *Rivista,* my courses, and by means of practically all of my work for the insane and for children of wayward character. Two rather original methods which are quite promising are the application of individual psychology to the insane and the treatment in common of mentally retarded cases and young people whom we call criminals.

The most important problem for me in the psychology of tomorrow is the unconscious, its educability, and the means for its education. The advantage of all of this is quite evident. Other important questions are: psychological heredity, the exact course of the educational process, the different values or the interdependence of the intellectual and the affective factors, and the progressive modifiability of character through the quasi-organic modifications of conduct.

It is impossible for anyone to foresee what will probably be the future goals of psychology. Anything is possible, but no one can prophesy what will happen, for elements that we cannot imagine (men of genius, events, the progress of science, or new discoveries) can influence the progress of scientific psychology because it is so new. The alienists are now in the process of observing the transformation which systematized delusions of persecution are undergoing under their very eyes due to the daily advances in science. Airships, radio technique, etc., make more and more real the various means of influence which these deluded persons imagine. Therefore, the latter see themselves obliged to change more and more the "foundations" of their delusions. I mention this observation simply for the purpose of insisting upon the impossibility of foreseeing the probable course of the interests of the generation which is to follow us.

If I could be born again just as I am, I should probably do again what I naturally have done during the life which is now drawing to a close. If I could choose, I should wish to be interested in and to be stimulated by the problems which made the lives of William James and Théodor Flournoy so happy.

SHEPHERD IVORY FRANZ

INTRODUCTION

According to many reviewers, when an autobiographer writes, with or without the help of diary notes, the ability of confabulation is his main literary asset. It is thought he cannot distinguish paramnesias from facts, and that he cannot hold himself free from paranoid interpretations. Evidences of flight of ideas, schizophrenic tendencies, delusions of persecution or of grandeur, as well as many other pathological states, may be found by different reviewers in the same paragraphs. The author, however, has the privilege of protecting himself by adopting the psychoanalytic principle of fixed symbolism and of using against his reviewers their apparent acceptance of this mode of interpretation. Thus both may be narcissisticly happy—each satisfied with his own opinions. Biography, whether of self or of others, is necessarily an account of believed facts and their interpretations. The history of peoples has shown that when two individuals (also nations) are given the same collection of facts they may differ radically in describing, interpreting, and in expressing opinions about them. The reader may, therefore, be advised that what follows is one way of looking at a series of occurrences.

EDUCATION AND GENERAL INFLUENCES

My beginning in psychology, at Columbia in 1892-1893, was an introduction to James's views as shown in his *Briefer Course* by way of J. H. Hyslop, who was at that time Instructor in Philosophy, Psychology, and Ethics (logic may also have been included in his title). What interest Hyslop had in the then modern psychology centered around the thought processes (e.g., logic). He published papers on visual space perception, besides a textbook on logic, a volume on Hume's Ethics, and, I think, a syllabus of psychology. Later he devoted himself to psychical research. I can recall distinctly, but when and under what circumstances I do not remember, his excellent advice in relation to my psychological work. He said he had formerly logically accepted a philosophical materialism, but that items in his experience precluded this, and he had therefore turned to its opposite, an idealism which is spiritualistic. He warned me not to accept a definite philosophy but to keep an open mind, to study what

I would, but to avoid metaphysical assumptions, because only in those ways was it possible to carry on one's scientific work to a satisfactory conclusion. That line of conduct has, I think, been followed.

Besides work with Hyslop, I took courses in the history of philosophy with Professor (now President) Nicholas Murray Butler, the most fluent and most polished university lecturer I have listened to. The scratchings of philosophical opinions were not deep enough to get me seriously infected, but on the contrary they probably were prophylactic and vaccinative. Butler's interest in the problems of education was more catching and I took additional courses in the history and in the philosophy of education, in child psychology and even in school administration, in partial satisfaction for a "minor" for the doctorate with him and with some of the faculty of the Teachers College. Anthropology was my second doctorate minor. I attended courses by Dr. Livingston Farrand (now President of Cornell University) on ethnology, by Dr. W. Z. Ripley on anthropogeography, and by Dr. Franz Boas on physical anthropology. Boas brought to us the methods of correlation, at that time, so far as I am aware, unapplied to psychological problems except in Galton's work.

In 1893-1894, as a college senior, I began my experimental work under Professor J. McKeen Cattell, and continued, as graduate student, fellow, and assistant, until 1899. Part of Cattell's story and influence has already been told[1] and doubtless more will follow, so that little is said here. Psychology was not an independent department. It was administratively combined with philosophy and with anthropology and remained so for a number of years. The elementary course was given in succession by several instructors who were more interested in the problems of ethics, metaphysics, and the history of philosophy than in psychology. What little interest they had in psychology centered in those mental phenomena which are supposed to be the basis for ontological and epistemological speculations. As I recall the situation, Cattell gave an introductory course in experimental psychology for college seniors, a seminar for graduates, and a research course. Farrand was giving courses in abnormal and physiological psychology (of which more later). Professor C. A. Strong was brought into the department in 1895. He gave a course on theoretical psychology which was the basis for a book published later under the title, *"Why the Mind has a Body."* At that time there were no courses in statistics or mental tests, no educational or child psychology apart from the courses in education, no courses in social, clinical, comparative, or applied psychology. The data for these special courses, which are being greatly stressed at the present time, were being collected.

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[1] Researches of J. McKeen Cattell. *Arch. Psychol.*, 1914, No. 30.

Cattell's course in experimental psychology followed what would now be called traditional lines, experiments on vision and the other senses, the perception of movement, association, memory, reaction-time determination, etc. With Farrand, he had begun his series of mental and physical tests to differentiate and predict student capabilities,[2] and in these tests all his graduate students were assistants. The laboratory, as in many other universities where it was not found in a basement, was located in an attic. The electrical apparatuses were operated by gravity cells which were a constant source of annoyance to all. One of my jobs was to look after them. Although I probably did it badly, I learned much about the general facts of physics, and especially of electricity, through consultations about my difficulties with Professor M. I. Pupin (since become famous for his work on long-distance telephony and for other electrical inventions) who had his laboratory in the basement of the same building as the psychological laboratory. Because my first research work was on vision, I also freely consulted with, and was much encouraged by, Professor Ogden Rood (then one of the leading physicists in the United States who dealt with color phenomena) and by Professor William Hallock also of the Department of Physics. Zoological work was carried on in the buildings of the College of Physicians and Surgeons about a mile and a half away from Columbia College and my relations to that department were not close. I began, but could not finish, a course in general zoology under Professor Edmund B. Wilson (known for his work on the cell), and attended a few lectures by Professor Henry Fairfield Osborn. The University was moved to its new site at 116th Street in 1897 and the department was more adequately housed (but, inconsistently with the few psychological traditions, neither in the basement nor in the attic) than in the old quarters.

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[2] Cattell, J. McK., & Farrand, L. Tests of college students; physical and mental measurements of the students of Columbia University. *Psychol. Rev.*, 1895, **2**, 618-648.

Among the students in the laboratory in the early years (to 1899) were Harold Griffing, C. Judson Herrick, E. L. Thorndike, L. B. McWhood, and R. S. Woodworth. They are the ones who most affected my work, but in quite diverse directions. Griffing had begun graduate work in philosophy but had transferred to psychology as a major. He worked mainly on haptics and published several papers. His doctorate thesis was on that subject.[3] In much of his work I acted as subject. Griffing was physically below par most of the time and died about 1898. Herrick's major work was in zoology but he assisted in a number of investigations. Thorndike came from Harvard in 1897 after the University had moved to its new site. He had taken courses with James, with Münsterberg, and with Royce, and had begun his work on animal psychology. For a time, until traditional attic quarters could be arranged for his work, his investigation was done in a few rooms about a half mile from the University buildings.[4] At the same time McWhood (who later became Instructor in Music under Edward McDowell) was carrying on work on tone perception. Woodworth had also come from Harvard, but with the added advantage of a year in physiology under Bowditch and Porter. His doctorate work on the accuracy of movement is well known.[5] Among the other graduate students in those days were: Breese (now at the University of Cincinnati), whose thesis work was on sensory inhibition;[6] G. V. N. Dearborn (who later taught physiology at Tufts Medical School and much later took up work as a physician with the U. S. Veterans' Bureau); Dexter, who wrote on the mental effects of the weather;[7] Lay, whose thesis was on imagery,[8] and who much later became interested in psychoanalysis.

An ad-interim spring semester at Leipzig in 1896 did not greatly influence me. Wundt was seldom seen except at the times of his lectures, but Meumann was more approachable and gave me much time, as did Mentz, who at that time was the second assistant. Judd was remaining in Leipzig to finish a translation of Wundt's introductory text, and Stratton had finished his doctorate work and was leaving. Although I was *Versuchsperson* in three of the investigations, the greatest value I received was the personal contacts, especially with Judd and Meumann. In the summer of that year I presented a paper on reading at the International Psychological Congress in Munich, where I met W. H. R. Rivers, of Cambridge. This led to a friendship lasting many years, unimpeded by the feeling of necessity for much correspondence.

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[3] Griffing, H. On sensations of pressure and impact. *Psychol. Monog.*, 1894, No. 1.

[4] Thorndike, E. L. Animal intelligence. *Psychol. Monog.*, 1898, No. 8.

[5] Woodworth, R. S. The accuracy of movement. *Psychol. Monog.*, 1899, No. 13.

[6] Breese, B. B. On inhibition. *Psychol. Monog.*, 1899, No. 11.

[7] Dexter, E. G. Conduct and the weather. *Psychol. Monog.*, 1898, No. 10.

[8] Lay, W. On mental imagery. *Psychol. Monog.*, 1897, No. 7.

A greater influence was the two years spent as assistant in physiology at the Harvard Medical School, where I went in 1899, after receiving the doctorate at Columbia. At that time I came under the influence of Henry P. Bowditch, then the dean of physiologists in this country, and especially of W. T. Porter. The latter was a most painstaking investigator, interested in the study of heart action, most generous of his time and encouragement to one who knew as little as I did of the fields of physiology in which he was interested, and one of the least self-seeking scientific men with whom I have come in contact. The late Allan M. Cleghorn was a co-assistant. He had worked in Great Britain and had been in touch with many investigations in physiology which subsequently developed importance when psychology became more physiological. It was he who introduced me to the subject now called endocrinology, to which he had been an early contributor. W. B. Cannon was then finishing some of his work on the movements of the gastro-intestinal tract. A Dutch visiting investigator also gave a turn to my work and thought, although his own research was at that moment apparently far removed from my interests. This was J. W. Langelaan, later Professor of Anatomy at the University of Leiden, now an investigator independent of university auspices, and known best for his studies on muscle tonus. I also absorbed an interest in chemistry, although without much technical knowledge of it, from those colleagues who were engaged in that field, especially A. P. Matthews, the late Waldemar Koch, and W. H. Parker.

At Dartmouth there were few personal scientific stimuli for me. President W. J. Tucker was a wholesome broadener, however much he lacked acquaintance with scientific needs. The necessity of teaching a course in "medical" physics brought me contacts with the physics-chemistry group of instructors, especially the late E. F. Nichols and Gordon F. Hull. Dr. Gilman D. Frost, Professor of Anatomy, supported my work and made the development of physiology possible. Subsequent personal influences will be considered in the sections devoted to research topics.

INVESTIGATION TOPICS

Vision, Movement, Etc.

Two general research fields of work (vision, movement) were brought to my attention when I was a graduate student at Columbia, and articles on both were subsequently published. The topic of visual after-images was assigned to me as a thesis topic. I was to investigate the relations of time, space, and intensity of light to the production (threshold) and character of the visual after-image. This line of work was finished in 1899[9] but unsatisfactorily to me because I am only slightly visual minded. To that field I have not recurred. During the early years I also undertook to work out with Griffing some of the conditions incident to rapid reading, following Cattell's early work on legibility of letters and type. That research was also published,[10] but in the more recent investigations of the topic it seems to have been overlooked, even though some of the studies have in a measure duplicated our earlier work. This may have been due to its title, which was "On the Conditions of Fatigue in Reading." The title, perhaps, does not convey the idea that we were concerned with different forms of type rather than with school problems. The objective measures and the practical applications of research topics which were possible in the latter work have kept me more interested. The relation of the after-image to, and its difference from, visual imagination has no appeal, largely, I fancy, because of my make-up.

The second general line of investigation was also a suggestion from Cattell. His work on movement, especially that on perception, is well known. His interest in the problems of fatigue is seldom recognized. His criticism of the earlier methods of Mosso and many others led to his use of another method to measure muscular fatigue and to investigate conditions which affect the phenomena. This was by the use of an extension spring instrument which would be capable of measuring those slight amounts of work which were not measurable by the use of a constant weight. Cattell's instrument was exhibited at early meetings of the American Psychological Association, and briefly described in the proceedings of that society. I assisted him in the investigations of the conditions affecting fatigue, but the work was unfinished when I left Columbia. The results of his studies, so far as I am aware, have not been published. At Harvard, Professor Porter advised me to continue work on movement and fatigue, with special reference to the results of physiologists on isolated muscles. This led to a comparison of methods of measuring muscular work, including the ergometer method of Cattell, and the conclusion was reached that the isometric method, which is approximated in the use of the oval dynamometer for the strength of hand-grasp, is the best.[11]

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[9] The after image threshold. *Psychol. Rev.*, 1895, **2**, 130-136. After images. *Psychol. Monog.*, 1899, No. 12. Pp. 61. On after images: an explanation. *Psychol. Rev.*, 1901, **7**, 63-64.

[10] With Griffing, H. On the conditions of fatigue in reading. *Psychol.* Rev., 1896, **3**, 513-530.

The studies on movement and fatigue (and I do not exclude the work of Woodworth)[12] led me, along with other considerations, to undertake later additional motor studies, relating both to psychopathological topics[13] and to cerebral functions. Doubtless my motor trend had greater influence to this end. The latter had much to do with the investigation of fatigue factors in occupation[14] and to the work of Bloedorn on occupation accidents which was done under my direction.[15] In this same way arose the impetus for the work of Kent[16] and of Boring [17] on habit formation in the insane, and my consideration of the broad aspects of the problem.[18] The general motor interest also led to the observations on handedness in monkeys.[19] It is certain that motor-mindedness influenced the direction of some of my investigations on cerebral functions[20] and of the allied studies by Lashley[21] and by Stout[22] which were advised by me.

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[11] On the methods of estimating the force of voluntary contractions and on fatigue. *Amer. J. Physiol.*, 1900, **4**, 348-372.

[12] Woodworth, R. S. The accuracy of movement. *Psychol. Monog.*, 1899, No. 13.

[13] Anomalous reaction times in a case of manic-depressive depression. *Psychol. Bull.*, 1905, **2**, 225-232. With Hamilton, G. V. The effects of exercise upon the retardation in conditions of depression. *Amer. J. Insan.*, 1905, **62**, 239-256. The time of some mental processes in the retardation and excitement of insanity. *Amer. J. Psychol.*, 1906, **17**, 38-68. The knee jerk in paresis. *Amer. J. Insan.*, 1909, **65**, 471-498. Fatigue factors in certain types of occupations. *Trans. XV Int. Cong. Hygiene*, 1913, **3**, 512-517.

[14] Fatigue factors in certain types of occupations. *Trans. XV Int. Cong. Hygiene*, 1913, **3**, 512-517.

[15] Bloedorn, W. A. Studies of industrial accidents which occurred in the Navy Yard at Washington, D. C. *U. S. Naval Med. Bull.*, 1916, **10**. Pp. 43.

[16] Kent, G. H. Experiments in habit formation in dementia praecox. *Psychol. Rev.*, 1911, **18**, 375-410.

[17] Boring, E. G. Learning in dementia precox. *Psychol. Monog.*, 1913, **5**, No. 63. Pp. 101.

[18] On the occupation of the insane. *Govt. Hosp. Insane Bull.*, 1913, No. 5, 9-20.

Cerebral Functions and Re-education

Quite a number of factors combined to direct my attention to the problems of physiological psychology, using that term in the sense of "the relation of mental conditions to the activities of the nervous system." In point of time, the first was the reading of parts of Wundt's *Physiologische Psychologie*. The second was a special course in physiological psychology taken under Dr. Livingston Farrand, then Instructor in the Department of Psychology at Columbia. A third was the necessity of reading both current contributions on brain functions and of reviewing much of the older classical literature in my first year at the Harvard Medical School. The last was, I think, the direct drive or urge in that direction. This was largely because of the conditions that confronted me. Having had a training in psychology, I was expected to know something about the brain, the organ of mind, as well as about the sense organs, the originators of mental states. Physiology, on account of the rapid developments in chemistry, was becoming, or had become, largely chemical, and more physiologists were concerning themselves with problems in that field—the digestive functions, the blood, questions of secretion, respiratory activities, and such topics as the relations of chemical substances to heart and muscle action. The great interest and the intensive work on the brain which began in 1870, with Fritsch and Hitzig, and which was continued through the eighties and nineties by Munk, Goltz, Ferrier, Schäfer, Horsley, and others had waned. It superficially appeared as if practically all the important work that could be done had been accomplished. The main facts had, it seemed, been discovered, and there remained only the finer determinations and the filling in of small lacunæ.

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[19] Lashley, K. S. Modifiability of the preferential use of the hands in the Rhesus monkey. *J. Anim. Behav.*, 1917, **7**, 178-186.

[20] With Scheetz, M. E., & Wilson, A. A. The possibility of recovery of motor function in long-standing hemiplegia. *J. Amer. Med. Asso.*, 1915, **65**, 2150-2154. With Stout, J. D. Variations in distributions of the motor centers. *Psychol. Monog.*, 1915, No. 81, 80-162. With Oden, R. On cerebral motor control: the recovery from experimentally produced hemiplegia. *Psychobiol.*, 1917, **1**, 33-49. Motor education (of the crippled child). *The Crippled Child,* 1928, **6**, 28-30. (Reprinted in the *Educ. Res. Bull.*, 1928, **7**, 2-4).

[21] Lashley, K. S. The accuracy of movement in the absence of excitation for the moving organ. *Amer. J. Physiol.*, 1917, **43**, 169-194.

[22] Stout, J. D. On the motor function of the cerebral cortex of the cat. *Psychobiol.*, 1917, **2**, 177-229.

As he was leaving for a semester in the fall of 1900 to go to Naples, Professor Porter had suggested that I might work on some problems connected with the nerve supply of the heart. The method suggested was followed in experiments for about six weeks, but without results, and I then discussed with Professor Bowditch the possibility of trying an experiment on the brain which had occurred to me. I outlined what I wanted to do, and his reply was to try it even though he thought the probability of success was slight. His subsequent pleasure when shown the preliminary experiments and results was not disguised, and he strongly advised devoting my time to that work. It was partly because of his interest that I received a grant from the Carnegie Institution of Washington for the continuation of the investigation.

I had read the account of experiments on the frontal lobes by Professor Bianchi[23] in which he cited the fact that a baboon which had acquired habits of saluting lost those habits following the removal of the frontal areas. Corresponding observations were, I think, reported also by Sir Edward Sharpey-Schäfer. It was these suggestions or stimuli which immediately gave rise to the idea of the use of the training method of investigating the functions of the frontal lobes. At the time Bianchi and Schäfer reported their findings there were not available the data on methods of animal learning which had developed in the hands of Lloyd Morgan and especially of Thorndike, but it was a simple step from the latter's results to try the combination of animal training and extirpation as a method whereby some additional cerebral problems might be solved. It seemed to me possible that the combination training-destruction method would give results of special value in relation to the association functions of the brain, but also in relation to the supposed sensory zones, which had not been adequately investigated, as evidenced by the Munk-Goltz controversy.

The application of the method was first made by me in the autumn of 1900 to the problem of frontal lobe function. The results of the early experiments on cats were reported in 1902[24] and confirmed on monkeys,[25] and similar results were reported by Swift. The work attracted the attention of Professor Edward A. Schäfer (now Sir Edward Sharpey-Schäfer) of Edinburgh, and of Professor (now Sir Charles) Sherrington, then at Liverpool. Both of them commended me by letters and encouraged me. That two men who had contributed so eminently to the subject of neurology regarded the work highly enough to write to me was an added encouragement, especially since at that time I had not been associated or become personally acquainted with either of them. A further and a much later commendation came in a less direct and in a less pleasant fashion. This was the appearance of an article by Kalischer in which he appropriated the training-extirpation method as his own. Of his publication, I was informed at the Heidelberg Physiological Congress in 1907. To it, I protested[26] because I could see no reason why the method, if of any worth, should be labelled "made in Berlin." Kalischer's article was, however, as complimentary as is all plagiarism.

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[23] *Brain,* 1894, **18**, 497-522.

General reviews and articles had been written[27] and others followed,[28] which enabled me to set down what others had been doing and to evaluate them mainly for my own purposes. Preparation of material for these general reviews prompted with further suggestions, but it is impossible to set forth except in this general fashion the matters which intimately drove in the directions which my later investigations of the cerebrum took.

Experiments on animals and humans were made as occasions arose, and hospital connections made many human cases available which would not have happened in a university or in a university research center. The functions of the post-central areas can be investigated in animals, but human subjects with disturbances in those areas have many advantages for the investigator. Such a one was investigated.[29] Although subsequently I carried out animal experiments on the occipital lobes,[30] I have always wished to have a few human cases in which there were accidental destructions in the calcarine areas. There is much to be done towards the clearing up of points connected with the so-called visual area beyond what Henschen and others have done, and I have the conceit that the human cases will reveal much when they are approached from a different point of view than that of Henschen and the earlier investigators.

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[24] On the function of the cerebrum: the frontal lobes in relation to the production and retention of simple sensory motor habits. *Amer. J. Physiol.*, 1902, **8**, 1-22.

[25] Observations on the functions of the association areas (cerebrum) in monkeys. *J. Amer. Med. Asso.*, 1906, **47**, 1464-1467. On the functions of the cerebrum: the frontal lobes. *Arch. Psychol.*, 1907. Pp. 64.

[26] Ueber die sogenannte Dressurmethode für Zentralnervensystemsuntersuchungen. *Zentbl. f. Physiol.*, 1907, **26**, 583-584.

[27] Localization of brain function. *Psychol. Rev.*, 1901, **7**, 418-426. The brain and the mind. *N. Y. Teach. Monog.*, 1902, **4**, 46-50.

[28] The functions of the cerebrum. *Psychol. Bull.*, 1911, **8**, 111-119. Physiology of the brain. *Ref. Hdbk. Med. Sci.*, 1913, **2**, 397-412. The functions of the cerebrum. *Psychol. Bull.*, 1913, **10**, 125-138. Physiology of nerves. *Ref. Hdbk. Med. Sci.*, 1914, **5**, 732-738. Functions of the cerebrum. *Psychol. Bull.*, 1914, **11**, 131-140. The functions of the cerebrum. *Psychol. Bull.*, 1916, **13**, 149-173.

That the studies of human cases in whom there are cerebral destructions have value for the understanding of the motor areas has been shown in the investigations of both animals and humans,[31] which have continued up to the present time. The general reinvestigation of the motor functions had its immediate rise from two sources. One of these was the chagrin in attempting to demonstrate to students cerebral motor function as they were taught in the textbooks of physiology and of psychology. The failure to be able to locate on the animal cortex the points from which definite movements were organized or originated led me to explore anew the motor area in the monkey brain, to plot the points, and to compare cortical areas in different animals. The brilliant results of Sherrington in this field were also suggestive in many directions. The second suggestive source was the feeling that even after we discount the differences in the directions and in the amounts of development of the cerebral cortex in such animals as rodents, carnivora, monkeys, and man, there should be a closer correspondence in certain of the elemental functions, especially the motor. For many years it had been known that the anterior part of the cerebrum of a rabbit (presumably including the structures which, if any, could be said to be concerned with cortical motor control) could be removed without seriously interfering with the animal's activity for more than a brief period. Similarly with the dog, and to a less degree with the monkey. Man was believed to be different quantitatively if not qualitatively. The investigation of some human hemiplegics showed a possibility of functional recovery after destruction of the cortical motor area, or its separation from the spinal centers, beyond what had previously been anticipated.[32] The paralyzed subjects could readily learn to carry out activities even of a highly complex character. On the other hand, when the precentral cortex in the monkey brain was destroyed, with resultant hemiplegia, it was found possible to bring about motor control within a brief period of time.[33]

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[29] On the functions of the post-central cerebral convolutions. *J. Comp. Neur.*, 1911, **21**, 115-127.

[30] On the functions of the cerebrum: concerning the lateral portions of the occipital lobes. *Amer. J. Physiol.*, 1911, **28**, 308-317. With Lafora, G. R. On the functions of the cerebrum: the occipital lobes. *Psychol. Monog.*, 1911, No. 56. Pp. 118. On some functions of the occipital lobes. *Govt. Hosp. Insane Bull.*, 1912, No. 4, 5-20.

[31] With Scheetz, M. E., & Wilson, A. A. The possibility of recovery of motor function in long-standing hemiplegia. *J. Amer. Med. Asso.*, 1915, **65**, 2150-2154. With Stout, J. D. Variations in distributions of the motor centers. *Psychol. Monog.*, 1915, No. 81, 80-162. With Oden, R. On cerebral motor control: the recovery from experimentally produced hemiplegia. *Psychobiol.*, 1917, **1**, 33-49.

The main amount of attention the latter series of results immediately attained was in the nature of adverse criticism. At first there was expressed grave doubt that in the human cases the precentral cortex was destroyed or its connections broken. The cases could not, in fact, be those of organic cerebral destructions. Next came the statements that the facts had, at any rate, been known for a long time. And within the past few years there have come the explanations of why we may expect functional return after cortical motor destruction. Within the past month, for example, I have been informed by a well-known neurologist that I must have forgotten, if I ever knew the fact, that phylogenetically the striate body acted long before the cerebral cortex developed, and there was no reason to believe that it had lost all its function in the course of development. These considerations would not be mentioned here were it not that they are interesting in relation to a more general matter which will be discussed later in this section.

Coincident with work on the frontal lobes, on the occipital lobes, and on the pre- and post-central areas, the subject of aphasia was also being studied. The direction of the inquiries was in looking at the recovery phenomena.[34] Although the recovery, or re-education, of speech function in aphasics has long been recognized as a possibility, it has been the subject of few special experimental investigations. When resumption of some speech has occurred, it has been assumed that the speech centers have not been destroyed—but only inhibited or affected in von Monakow's sense of diaschisis. When recovery has not taken place, or there has been very little, the assumption has been made that the vocalization or understanding speech centers have been destroyed. The contributions of Pierre Marie, and, more recently, of Henry Head, have changed somewhat the former generally accepted views of the relation of the cerebrum to speech functions and losses, but they have not added to our understanding of why patients do or do not have its reestablishment. In fact, the phenomena of relearning have received little attention from neurologists, at any rate, as far as publications are concerned. My investigations have shown that, in general, speech is slowly reacquired by an aphasic, but that some items are readily learned. In this respect cerebral speech function resembles cortical motor function. Perhaps also we should look at that reacquisition of speech somewhat differently than from quantity and temporal angles.

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[32] With Scheetz, M. E., & Wilson, A. A. The possibility of recovery of motor function in long-standing hemiplegia. *J. Amer. Med. Asso.*, 1915, **65**, 2150-2154.

[33] With Oden, R. On cerebral motor control: the recovery from experimentally produced hemiplegia. *Psychobiol.*, 1917, **1**, 33-49.

Some of the general problems of the cerebrum were also being investigated. Lashley and I tested rats with cerebral extirpations of different extents[35] and we discovered how little influence any one portion of the brain exerted. These studies have been ably continued by Lashley and the account of his brilliant work has recently been published. Here also should be mentioned Lashley's work on salivary secretion in hemiplegics,[36] a study undertaken to discover some of the more general phenomena associated with cerebral defect.

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[34] The re-education of an aphasic. *J. Phil., Psychol., etc.*, 1905, **2**, 589-597. On certain fluctuations in cerebral function in aphasics. *J. Exper. Psychol.*, 1916, **1**, 355-364. Studies in re-education: the aphasias. *J. Comp. Psychol.*, 1924, **4**, 349-429.

[35] With Lashley, K. S. The retention of habits by the rat after destruction of the frontal portion of the cerebrum. *Psychobiol.*, 1917, **1**, 3-18. With Lashley, K. S. The effects of cerebral destructions upon habit-formation and retention in the albino rat. *Psychobiol.*, 1917, **1**, 71-139.

[36] Lashley, K. S. Changes in the amount of salivary secretion associated with cerebral lesions. *Amer. J. Physiol.*, 1917, **43**, 62-72.

The early work on the frontal lobes of cats[37] had also brought forth items which had been unsuspected, and those items developed into a general line of investigation. They have compelled us to look at cerebral functions in a way different from that which was current in 1900. The items to which I refer are those relating to the possibility of recovery of functions which cerebral destructions appeared to abolish. Some of the items may have been known prior to 1900, but it is certain that the then current conceptions of the working of the brain were not much influenced by any general or wide-spread acceptance of the facts. These items led to what I consider a more adequate view of the way in which the brain works.

A series of articles has dealt with the general theory of cerebral function.[38] The first of these considered the differences in the frontal and posterior association areas. The facts point to the following conclusion: the frontal seems to be more intimately bound up with emissive activities, the posterior with the receptive, although it also seems probable that both function together in most of our cerebral associative states. The second general article[39] was a critique of the prevailing view, because it appeared to me to have followed the pigeon-hole conception of the earlier investigators who knew little of brain anatomy or brain physiology, and less of psychology. Those who protested against my presentation, either by publication or by personal communication, were more voluble in the matter than those who approved. I have never been interested in polemic and the critiques were not answered except indirectly by much later publications. Incidental discussion of the theoretical meanings of observational data[40] and special summaries of collected evidence from several directions followed.[41] Everything tended to show that there are not the definite and exact functions for parts of the cerebrum which were posited, but that there is rather a possibility of substitution. This does not mean that there is no localization, and no possibility of localization, of function in the cerebrum, but that there is not a localization in the sense in which such localizations were reported. The problem of cerebral function is not solved, either in a general or in a specific way. It remains a problem, not only of what, but also of how far, and how well defined.

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[37] On the function of the cerebrum: the frontal lobes in relation to the production and retention of simple sensory motor habits. *Amer. J. Physiol.*, 1902, **8**, 1-22.

[38] On the association functions of the cerebrum. *J. Phil., Psychol., etc.*, 1910, **7**, 673-683. New phrenology. *Science*, 1912, **35**, 321-328. Symptomatological differences associated with similar cerebral lesions in the insane. *Psychol. Monog.*, 1915, **81**, 1-79. With Stout, J. D. Variations in distributions of the motor centers. *Psychol. Monog.*, 1915, No. 81, 80-162. Cerebral adaptations vs. cerebral organology. *Psychol. Bull.*, 1917, **14**, 137-140. Cerebral-mental relations. *Psychol. Rev.*, 1921, **28**, 81-95. Conceptions of cerebral functions. *Psychol. Rev.*, 1923, **30**, 438-446. Nervous and mental reeducation. New York: Macmillan, 1923. Pp. ix+225. How the brain works. (Univ. Calif. at Los Angeles Faculty Research Lecture No. 2, 1926). Los Angeles: Univ. Calif. at Los Angeles, 1929. Pp. 35.

[39] New phrenology. *Science*, 1912, **35**, 321-328.

It is not surprising perhaps that the findings relative to the frontal lobes, to the motor areas, and to the speech centers should have given rise to a more intense interest in the general and specific problems of re-education. The outcome was a small book[42] which dealt with what I think are the five main types of defective states amenable to re-education procedures. During the World War and after there was much talk about the re-education of the injured. The largest part of it was only an expression of maudlin sympathy or of self-aggrandizement desire. There was, and is, much need for investigation of methods and of possibilities, but the need was not, and is not now, understood. Financial provisions for the education of the handicapped is being made, but there will remain a mass of trial-and-error efforts repeated in many communities, rather than a series of scientific experiments. The latter would tend to limit the application of the trials and to discover the conditions of the errors in the handling of large numbers of children and adults. Research programs were advocated but no opportunity to carry them out was forthcoming.[43] A generalized view of the problems involved in rehabilitation was set forth,[44] at the instigation of the late E. E. Southard who recognized the need of some kind of foundation on which the different, but related, structures could be built for an adequate rehabilitation venture. Semi-popular appeals may be helpful.[45] Much is being learned of the necessity for further investigation, and progress beyond what we now know is almost an actuality. This is largely because many communities are attempting to make their handicapped, especially the children, relatively independent, rather than to keep them in a dependent state. It is hoped that more can be done, but that "more" must necessarily involve careful and extensive—as well as immediately expensive—investigation.

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[40] Symptomatological differences associated with similar cerebral lesions in the insane. *Psychol. Monog.*, 1915, **81**, 1-79. With Stout, J. D. Variations in distributions of the motor centers. *Psychol. Monog.*, 1915, No. 81, 80-162.

[41] Cerebral adaptation vs. cerebral organology. *Psychol. Bull.*, 1917, **14**, 137-140. Cerebral-mental relations. *Psychol. Rev.*, 1921, **28**, 81-95. Conceptions of cerebral functions. *Psychol. Rev.*, 1923, **30**, 438-446. How the brain works. (Univ. Calif. at Los Angeles Faculty Research Lecture No. 22, 1926.) Los Angeles: Univ. Calif. at Los Angeles, 1929. Pp. 35.

[42] Nervous and mental re-education. New York: Macmillan, 1923. Pp. ix+225.

[43] Report of a conference on the re-education and rehabilitation of maimed and crippled soldiers. *Psychol. Bull.*, 1917, **14**, 229-232. Re-education and rehabilitation of crippled, maimed and otherwise disabled by war. *J. Amer. Med. Asso.*, 1917, **69**, 63-64. Report of Committee on Re-education Research. *Psychol. Bull.*, 1917, **16**, 416-418.

Psychopathology

My introduction to work in the field of abnormal psychology was not due to an early organized interest. For me it was developed from an accident. At Columbia, in 1893 or 1894, I had attended lectures on the subject given by Dr. Livingston Farrand. Farrand had just returned from Berlin where he had studied in the Psychiatric Clinic at the Charité. That course, as I now recall it, followed the lines we call psychiatry, without, however, an emphasis upon the mode of treatment. Kraepelin's work and teachings had not become dominant as they did at a later date, and there was more of the symptomatic than of the etiologic in Farrand's lectures. It was, therefore, more psychological than many of the courses in so-called abnormal psychology today. Subsequently, I had the opportunity of attending some of the lectures of Dr. Frederick Peterson, and a few of his clinics at Ward's Island, as well as clinical neurological lectures by Dr. M. Allan Starr.

During the next decade the problems of the mentally abnormal did not recur. However, in the fall of 1903, when I was an instructor in physiology in the Dartmouth Medical School, I became acquainted with Dr. Edward Cowles [Superintendent of the McLean Hospital (for the insane), Waverly, Massachusetts], who was then giving his annual course of lectures on psychiatry at Dartmouth. He brought to my attention Kraepelin's newer conceptions of the manias and melancholias, which were being looked upon as one disease, not different diseases. Dr. Cowles was attempting to define more accurately his personal views of the relationships of the excitements and depressions. He thought they were mainly fatigue situations, and he was trying to bring the variety of symptoms into relation with the newer ideas of nerve physiology as represented by Sherrington's *Integrative Action of the Nervous System.* Informally he asked me to outline an experimental plan whereby some of the problems might be attached. This I did briefly and in the rough, since my knowledge of the problem was not intimate and direct. Letters and a number of visits in which the matter was discussed further resulted in his securing funds for the prosecution of the suggested investigations for a period of three years. Although in the interval Dr. Cowles had retired from the superintendency and had been succeeded by the late Dr. George T. Tuttle, I was asked to undertake the work (1914). The plans had been precipitately pushed to forestall my acceptance of an offer to teach the physiology of the nervous system in another university.

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[44] Rehabilitation and re-education—physical, mental, and social. *Ment. Hygiene*, 1919, **3**, 33-47.

[45] Re-education of the injured brain. *Calif. Mo.*, 1926, **19**, 313-314. Motor education (of the crippled child). *The Crippled Child*, 1928, **6**, 28-30. (Reprinted in the *Educ. Res. Bull.*, 1928, **7**, 2-4.)

At that time the McLean Hospital was widely recognized for its advanced methods of dealing with patients and for its support of research dealing with the problems of psychiatry. The cottage system of segregating patients, the general use of nurses instead of untrained attendants, and the broad scientific outlook were well known. Dr. Otto Folin, now Professor of Biological Chemistry in the Harvard Medical School, was carrying on metabolism studies, and the late Dr. August Hoch, who had worked with Kraepelin on problems of fatigue and who had planned to continue similar laboratory investigations at the McLean, was then devoting himself to clinical studies.

The first psychopathological problems to be undertaken were in line with the interests and ideas of Cowles, and the studies led to two articles.[46] At the same time additional work was in progress in a number of other lines,[47] of which that on the cerebrum and that on re-education in aphasia are mentioned in the appropriate section.

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[46] Anomalous reaction times in a case of manic-depressive depression. *Psychol. Bull.*, 1905, **2**, 225-232. The time of some mental processes in the retardation and excitement of insanity. *Amer. J. Psychol.*; 1906, **17**, 38-68.

[47] With Hamilton, G. V. The effects of exercise upon the retardation in conditions of depression. *Amer. J. Insan.*, 1905, **62**, 239-256. The physiological study of a case of migraine. *Amer. J. Physiol.*, 1907, **19**, 14-38.

In 1904 I was practically ignorant of the problems and management of insane patients, but in the succeeding three years I acquired a working knowledge of current psychiatry and a more intimate acquaintance with the facts of abnormal psychology. In 1904 the work I was asked to do was rather grudgingly accepted by some of the staff, possibly because it had been precipitately organized, but when I left the McLean to go to Washington, Dr. Tuttle asked me to nominate a successor. The psychological laboratory approach had, in the period of three years, been generously accepted, and Dr. F. Lyman Wells, whom I nominated, became my successor (1907-1921).

During my final year at the McLean, Hoch and I worked together upon the analysis of the defects in aphasia and upon memory and attention conditions in senile and presenile patients. Both studies were incomplete at the time I left the McLean Hospital (1906), and it was planned to divide our further effort, Hoch to finish the aphasia study and I to work on memory. Neither piece of work as originally planned was brought to a close, although fifteen years later, Liljencrantz, one of my students, completed a corresponding study[48] in which the problem of senile memory defects was attacked from a somewhat different angle.

During my McLean residence the series of general articles on psychology and medicine was begun.[49] There had been some sporadic attempts in this country to take psychology into the clinics, and to introduce the facts of abnormal psychology to psychologists, but they were abortive (e.g., the work of Boris Sidis at the original New York Pathological Institute which is now almost forgotten). That psychiatry and other medical topics had developed and were being thought of anatomically (and only partly physiologically) was almost self-evident, and it was also self-evident that the psychologists of that date had not taken much account of the facts of abnormal states accompanying mental diseases. My position was unique. To some, both psychologists and psychiatrists, it was an invitation for attack, and to others a warning to defend themselves. Psychiatrists considered me to be a psychologist, lacking the general medical knowledge and experience for psychiatry. Some psychologists said I was not a psychologist. Obviously I tended to occupy a "No Man's Land," with whatever advantages that position conferred. The articles I published dealt with the general relations of psychology to medicine[50] and with the special relations of psychology and psychiatry.[51] It is likely, as one of my sponsors expressed it, that election in 1908 to honorary membership in the American Medico-Psychological Association (now the American Psychiatric Association) was due to my being looked upon as the protagonist of the rapprochement of psychology and psychiatry. The election was the first of a "non-medical" character for many years. At the present time there is a rather better general understanding of psychological-psychiatric relations, but whether due to, or in spite of, my activities must remain an open question. I do not claim credit, and I disclaim responsibility, for much of the development. That psychologists have become more interested in the problems of the abnormal is evidenced by the fact that in the past few years there have been a number of incomplete psychiatric texts written by them under the name of abnormal psychology. Psychiatrists, at the same time, have been reverting more to a symptomatic consideration of psychotic patients due to the incomplete psychological formulations of Freud and his followers, or his opponents.

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[48] Liljencrantz, J. Memory in the organic psychoses. *Psychol. Monog.*, 1923, No. 143.

[49] Psychological opportunity in psychiatry. *J. Phil., Psychol., etc.*, 1906, **3**, 561-567.

During the time these propaganda articles were being published there was a continuation of investigations, and their publication, in a number of fields. The diversity was due to many circumstances. Research work and its publication were, however, not the only matters to occupy me. When I went to Washington in 1907, it was with divided responsibilities and with divided income from two sources, from St. Elizabeth's Hospital (then officially known as The Government Hospital for the Insane) and from George Washington University. At the latter institution I succeeded as Professor of Physiology a practicing surgeon, just as, about six years previously, I had succeeded a practicing surgeon as Instructor in Physiology at Dartmouth. From that time on there was much instructional work for me both in the University and in the Hospital. Teaching at the University affected investigation only by consuming time, but at the Hospital the problem was different. I found a hospital that had existed for many years mainly as a custodial institution. There was more than ten times the number of patients than the McLean had, but there was not more than double the number of members of the staff to care for them. There had been four publications during four years by members of the staff, excluding the superintendent, Dr. W. A. White, and the pathologist, Dr. I. W. Blackburn. (In the next four years, excluding the same individuals, there were fifty-nine articles by thirteen members of the staff.) Attempts had been made by the recently appointed superintendent, Dr. W. A. White, to improve the clinical situation, and many of the staff were eager to learn. As an example of this, it may be mentioned that two of the physicians volunteered to help me unpack the collection of psychological apparatus accumulated by Dr. Arthur MacDonald for the U. S. Bureau of Education, which had been loaned to us for the inauguration of the psychological laboratory in the hospital.

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[50] Psychology and the medical school. *George Washington Univ. Bull.*, 1908, **6**, 7-15. The present status of psychology in medical education and practice. *J. Amer. Med. Asso.*, 1912, **58**, 909-911. On psychology and medical education. (Report of Committee.) *Science*, 1913, **38**, 555-566.

[51] On the development and needs of modern psychiatry. *Bull. Ontario Hosp. Insane*, 1908, **2**, 40-62. Examination of the insane. Chap. 7 in *Outline of psychiatry*, by W. A. White. New York, 1908. Pp. 65-93. The functional view of the insanities. *Govt. Hosp. Insane Bull.*, 1909, No. 1, 30-42. Handbook of mental examination methods. *Nerv. & Ment. Dis. Monog.*, 1912, No. 10. Pp. 165. Handbook of mental examination methods. (2nd ed.) New York: Macmillan, 1919. Pp. 193. On the occupation of the insane. *Govt. Hosp. Insane Bull.*, 1913, **5**, 9-20. The functions of a psychologist in a hospital for the insane. *Amer. J. Insan.*, 1916, **72**, 457-464. Psychology and psychiatry. *Psychol. Bull.*, 1917, **14**, 226-229. Psychology and psychiatry. *Psychol. Rev.*, 1922, **29**, 241-249. Mental traumata and the preparation of the medical profession to care for them. *Int. Clin.*, 1923, **4**, 1-8.

My first job was the preparation of a clinical examination procedure which could be used as a basis for the routine examination of patients. This was adopted for use in the Hospital in the spring of 1907, subsequently published,[52] and then expanded in 1912 into a book.[53] Much routine clinical work was done, two psychiatric articles were published with one of the ward physicians;[54] considerable editorial duties were carried on, but in the early years more attention was directed to the examination of organic neurological cases as well as to the functional psychotic, because of their relations to the general research activities in the Hospital and their value in my own cerebral investigations. In addition, Dr. I. W. Blackburn, and especially Dr. Nicolas Achucarro (who had been an assistant to Ramon y Cajal), gave me desultory instructions in neuropathology and, much more, encouragement in my work in clinical and experimental neurology.

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[52] Examination of the insane. Chap. 7 in *Outline of psychiatry,* by W. A. White. New York, 1908. Pp. 65-93.

[53] Handbook of mental examination methods. *Nerv. & Ment. Dis. Monog.,* 1912, No. 10. Pp. 165. Nervous and mental re-education. New York: Macmillan, 1923. Pp. ix+225.

[54] With O'Malley, M. On a case of polyneuritis, of autotoxic origin, with Korsakow's symptom-complex, with autopsy and microscopical findings. *Amer. J. Insan.,* 1908, **65**, 269-291. With O'Malley, M. A case of delirium produced by bromides. *Govt. Hosp. Insane Bull.,* 1909, No. 1, 82-88.

Two articles of great importance in their general application to many hospital problems were published by assistants and should be mentioned here. Both deal with the problem of habit formation in the large dementia precox group of custodial patients.[55] These investigations showed the fundamental facts in habit formation in one mental disease which had been looked upon as incurable, and they laid the foundation on which to build for the benefit of an institution as well as a patient. The work of Liljencrantz, mentioned above, was also a corresponding, but different, beginning at an approach to the better adaptation of senile and presenile patients. Boring's article on introspection in dementia precox[56] was an attempt to bring the more objective work with psychotic patients into relation with the normal by the time-honored method demanded by Wundt, and especially emphasized by Titchener.

During the last fifteen years of my St. Elizabeth's service there was a volcanic rise of psychoanalytic belief. Tedious laboratory studies were looked upon as unfruitful, if not entirely useless. Even the organic neurological had assumed value only if correlated with Freudian mental mechanisms. There was a bewildering stream of psychoanalytic outpourings, but fixed symbolism was the lava binding everything together. Whatever the formulation, and however it might be verbally described at different times by reference to organic conditions—the autonomic system, short aortas, and the like—the trend remained much the same. Diverse activities—research on the cerebrum and on re-education, war work, multiple teaching duties, and administrative details—fortunately (or the reverse) kept me away from the main volcanic outpourings and I did not become submerged by them. Nor did I attempt to stem or direct the flow. I was an onlooker.

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[55] Kent, G. H. Experiments in habit formation in dementia praecox. *Psychol. Rev.*, 1911, **18**, 375-410. Boring, E. G. Learning in dementia precox. *Psychol. Monog.*, 1913, **5**, No. 63. Pp. 101.

[56] Boring, E. G. Introspection in dementia precox. *Amer. J. Psychol.*, 1913, **24**, 145.

In more recent years no research project in abnormal psychology has been carried out with psychotic patients. I have been engaged only with those who showed abnormalities of movement or of sensation dependent upon organic nervous disturbances and who were re-education problems, and with those who exhibited intellectual and other disturbances in relation to their academic work.[57] The term "mental hygiene" has most often been used to designate this latter activity. Much material in this field is not yet published. Two earlier articles on the association-word experiment were published because of their relation to psychopathological conditions.[58] The work was done because of an interest in the reactions of the insane and also those of criminals, who made up a special group at St. Elizabeth's. Four other general articles have dealt with the special problems in abnormal psychology, respectively, on cranks, on delusions, on different kinds of abnormal individuals, and on the education of the subnormal.[59] These also may, although in a modified sense, be considered propaganda.

Skin Sensations

Several times I have been asked how it came about that, in the midst of investigations on cerebral functions and on psychopathological topics, problems on touch and other skin sensations were also investigated. If I had not had an early and relatively intimate acquaintance with work in this field I probably would not have had my attention directed to it by a number of abnormal conditions with which my other work brought me in contact. In 1893-1895 I had been a subject in some of Griffing's investigations on haptics to which references have been made above. I had retained a reading interest in the work, and later this increased because of possible relations to psychopathological phenomena, especially to the group of somatic delusions and hallucinations. It was the general interest which led to the investigation of a case of migraine while I was at the McLean Hospital[60] and to other investigations on pain sensations as illustrated by a case the history of which I published later with O'Malley.[61]

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[57] Problems of student adjustment in the university. *J. Delinq.*, 1925, **9**, 131-137. Student personnel problems. *J. Delinq.*, 1926, **10**, 519-524. The problem of the unsocial student. *Calif. Mo.*, 1928, **21**, 479-480.

[58] With White, W. A. The use of association tests in determining mental contents. *Govt. Hosp. Insane Bull.*, 1909, No. 1, 55-71. Some considerations of the association-word experiment. *Govt. Hosp. Insane Bull.*, 1910, No. 2, 73-80.

[59] Psychological opportunity in psychiatry. *J. Phil., Psychol., etc.*, 1906, **3**, 561-567. Delusions. *Pop. Sci. Mo.*, 1915, **86**, 80-91. The abnormal individual. Chap. 21 in *Foundations of experimental psychology*, ed. by C. Murchison. Worcester, Mass.: Clark Univ. Press; London: Oxford Univ. Press, 1929. Pp. 809-831. Education of subnormal and feebleminded. In *Encyclopaedia Brittanica*, 1929, 14th ed., vol. 21. Pp. 499-500.

About 1908, Achucarro and I conducted a series of general neurological examinations of a large group of patients who were without obvious organic neurological disturbances. Some of them had shown evidences of what may be called proprioceptive hallucinations and delusions. These patients mere mainly of the dementia precox and cyclothymic types. When the sensory findings were transferred to charts, some peculiarities became obvious. Apparent hyperesthesias and apparent hypoesthesias in special regions, e.g., abdomen, chest, or thighs, were so frequently encountered that psychoanalysts to whom the results were exhibited explained them immediately, and adequately, to their own satisfaction. The discovery of these peculiarities coincided in time with some clinical examinations of the sensory losses in the forearm and hand of an attendant whose ulnar nerve had been accidentally cut by a patient. The examinations of this man led to more careful experiments than were needed for clinical purposes because the results did not entirely coincide with those reported by Head, Rivers, and Sherren in similar cases.[62]

The psychoanalytic explanations of the sensory findings in the psychotic patients were not satisfactory to us, because data for comparison with sensations and reactions of normal persons were not available. I could not tell whether or not a seeming state of relatively slight hyperesthesia was normal, for example, for the thigh as compared with the leg or abdomen. Because of the lack of data on the normal variations in skin sensitivity, I reported a series of threshold determinations for ninety-five bodily points on each of five subjects.[63] Subsequently, additional work was done on the differences in localization ability which was thought to be indicative of degrees of sensitivity.[64] At the same time additional observations were made on a number of patients with organic cerebral disturbances, and one of these studies was published.[65]

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[60] The physiological study of a case of migraine. *Amer. J. Physiol.*, 1907, **19**, 14-38.

[61] With O'Malley, M. On a case of polyneuritis, of autotoxic origin, with Korsakow's symptom-complex, with autopsy and microscopical findings. *Amer. J. Insan.*, 1908, **65**, 279-291.

[62] Sensations following nerve division: I. The pressure-like sensations. *J. Comp. Neur.*, 1909, **19**, 107-123. II. Sensitivity of the hairs. III. Temperature sensations. *J. Comp. Neur.*, 1909, **19**, 215-235.

The three studies, on intensity threshold and on localization, showed considerable individual variations. These variations were not in consonance with results reported by Ponzo, nor with more recent studies by Cole. A re-investigation now in progress, of the problem of touch localization with special references to cerebral conditions, has so far confirmed the results reported in the 1913 and 1916 articles, but none of the studies has afforded a clue to a solution of the problems of the sensory differences in the functional psychoses.

A different line of approach was projected, and a series of experiments planned, to get data which might be helpful in explaining some of the phenomena. This was the determination of some of the factors which in a normal individual might bring about variations from the normal skin sensations. Changes in the blood supply, cold and heat, and local anaesthetic agents were briefly tested, but only one paper was published, with W. C. Ruediger.[66] Nor did these inquiries reveal the clue which was sought. The original problem remains as it was. I have no hope now that I shall be able to explain the phenomena, but the investigations have at least kept me away from a too-exclusive research program.

L'Envoi

The account is written. I am sure it cannot be balanced. Of necessity, I have more data regarding what I have paid than regarding what has been given or loaned to me. If there be one who thinks I have not acknowledged to him a just debt, I trust I may be given the opportunity for its acknowledgment. I should like to feel that the account shows more credits than debts, but the Future, the expert accountant, can alone determine that.

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[63] Touch sensations in different bodily segments. *Govt. Hosp. Insane Bull.*, 1910, No. 2, 60-72.

[64] The accuracy of localization of touch stimuli in different bodily segments. *Psychol. Rev.*, 1913, **20**, 107-128. The constant error of touch localization. *J. Exper. Psychol.*, 1916, **1**, 83-98.

[65] On the functions of the post-central cerebral convolutions. *J. Comp. Neur.*, 1911, **21**, 115-127.

[66] With Ruediger, W. C. Sensory changes in the skin following the application of local anesthetics and other agents: I. Ethyl-chloride. *Amer. J. Physiol.*, 1910, **27**, 45-59.

If I were called upon to dedicate a work to the one to whom I was most indebted, I could only draw straws to make the selection. My indebtedness has consisted not so much of quantity as of quality. Psychologically, as well as financially, redness and sweetness cannot be balanced. Gladly, however, I list my chief living creditors, without any implication of preferment: J. McKeen Cattell, Livingston Farrand, Nicholas Murray Butler, William T. Porter, John B. Watson, Sir Edward Sharpey-Schäfer, Sir Charles S. Sherrington, Henry Head, and my own students and assistants. Many of them have been barely mentioned in the foregoing account. Their influence has been none the less real and constant. I have not been privileged to know all of them well in a personal way. I have been privileged to know them all well through their publications as teachers and scientific stimulants.

KARL GROOS*†

Since it is not always easy for a scientist to indicate his own proper place in his profession, I am glad to be able to begin with a view presented by a colleague. In the *Einführung in die Psychologie*, published by Emil Saupe in 1927, the psychologist and philosopher, Carl Siegel, now working in Graz, has a section on the "naturwissenschaftlich fundierte Psychologie des Geisteslebens" in which the efforts of Höffding, Jodl, James, and myself are discussed. The heading is meant to indicate that the four authors named belong together because they have directed their interest toward philosophical studies such as ethics, the philosophy of religion, and aesthetics, but, at the same time, in their methodology, use the point of view of biological psychology, being convinced that the mental must be regarded as the highest phase of development of organic life. Siegel adds that, in this connection, I show a close relationship to James. I can corroborate this statement, for, in my epistemological attitude, also, I may add in passing, I was stimulated to interest in the problem of knowledge by the well-known book *The Will to Believe*, without, however, necessarily agreeing with pragmatism on this account.[1]

In looking back now upon the temporal sequence of my publications, I find that I began, as a young instructor, with the attempt to treat aesthetics from psychological points of view. The course of development which started then has never been completely interrupted. Twice in the course of my life, however, special turnings appeared which were related to the method of work and directed my view beyond the old problems toward new psychological problems also. The first time, there was the wish to use in my investigations, so far as possible, the experimental methods fostered in Germany. Then a new tendency became active when I undertook to develop the psychological treatment of literary documents more than this had been done heretofore. I wish to show this briefly in an introductory survey, and then to explain more systematically the separate contributions to the understanding of mental life which seem to me worthy of mention. It is only in connection with the experiments on the psychology of thinking that I shall present any results in this first section.

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*Born December 10, 1861, in Heidelberg.

†Submitted in German and translated for the Clark University Press by E. Marion Pilpel.

[1] Cf. my essays: "Was ist Wahrheit?" (*Int. Woch.*, 1910); "Ueberzeugung und Wahrheit." (*Phil. u. Leben*, 1925); and "Die Sicherung der Erkenntnis." (Tübingen: Osiandersche Buchhandlung, 1927.)

I

During my student period in Heidelberg (1880-1884) I attended principally Kuno Fischer's lectures on the history of philosophy, and psychology was rather foreign to me. My first book, which made possible my admission to the faculty of Giessen, accordingly treated a subject of quite another sort, namely, Schelling's *Reine Vernunftwissenschaft* (1889). It was not until I was already a docent that the problems of psychology began to grip my attention more strongly. As Siegel indicates, however, this occurred through aesthetics. The attempt which I made in my *Einleitung in die Aesthetik* (1892) to determine the nature of the beautiful and of the so-called aesthetic modifications was based upon a psychological analysis of subjective behavior in the enjoyment of art and nature. It seemed to me that this behavior should be regarded as a sort of play, which I designated as the "Spiel der inneren Nachahmung." In the book *Der ästhetische Genuss* (1902), which appeared instead of a second edition of the *Einleitung,* the same view is more specifically grounded. This indicates the point which became central for a large part of my literary activity. The fact that I brought the "innere Miterleben" of the aesthetically appreciating individual, emphasized particularly by Lotze, into relation with play, on the one hand, and the instinct of imitation, on the other, showed my inclination toward a biological point of view in psychological investigation. It was therefore in the natural course of further development that contributions to genetic psychology grew out of my conception of aesthetic behavior. Among these contributions belong the books on *Die Spiele der Tiere* (1896; 3rd ed., 1930), *Die Spiele der Menschen* (1899), also *Das Seelenleben des Kindes* (1903; 6th ed., 1923), and a rather long series of essays some of which I shall mention later.

It was not until I reached Basel, where I was Professor from 1898 to 1901, that the methodological interest began which I shall discuss next. I then began to occupy myself with experimental investigations which I continued after I had returned to the University of Giessen, where I now also had to give lectures on pedagogy. Since I had not been trained in any experimental institute, I limited myself, conscious of this lack, to problems which in my opinion could at least be attacked, if not completely solved, without the use of complex apparatus. Thus it was that even in Switzerland I had begun experiments on certain errors involved in the reproduction, from memory, on paper, of simple geometric figures which were laid before the subjects for a few seconds but were removed from view before the reproduction. In *Das Seelenleben des Kindes* (6th ed., pp. 134 ff.), I describe the results obtained in school classes by me and my students. They could probably be improved and amplified through the systematic utilization of introspection introduced later by Külpe. One conjecture which I expressed seems to me to merit further consideration in relation to the findings—consideration, however, in which the theory of reproductive types would have to be taken into account. This conjecture is that in such errors (more accurate knowledge of which would probably be not without importance for the teaching of drawing) not only the visual impression, but also the motor impulses aroused by this impression, might acquire influence upon reproduction.

More important, in my opinion, are the experiments on thinking (*Denktätigkeit*) which were likewise begun in Basel and continued later. My *Experimentelle Beiträge zur Psychologie des Erkennens* which were published in 1901 and 1902 in the *Zeitschrift für Psychologie* belong, with Karl Marbe's essay on judgment, directed to other ends, among the earliest efforts to let the experimental methods which Ebbinghaus had already expanded for use on memory penetrate into the field of intellect also. These experiments, continued by many students, bore particularly upon the progress of the child in the posing of questions, upon the development of interest in causal thinking, and upon the relative strength of this interest within the manifold mental connections which, in logic and epistemology, are designated as categories but which are here regarded from purely psychological points of view. I shall give the essential points now, as the method of procedure will be clarified in this way.

First the matter of the questions. There was one series of experiments to which notably H. Grünewald, a teacher, then working in Herborn, made an important contribution. In this series, there were read aloud to pupils of various classes, also to university students, sentences which were intended to stimulate the asking of questions—sentences such as the following: "In the jeweler's show-window there is a stone of great beauty," or "Suddenly the cry of 'Fire!' resounded in the village." After the reading of such sentences, the subjects were asked to indicate, by means of questions which they wrote down, what else they would like to know about what they had been told. These questions revealed a distinction, already known from grammar, which, psychologically considered, is important in the working of intelligence and phantasy. There are questions of determination (*Bestimmungsfragen*), which cannot be answered with "yes" or "no" because they represent, as it were, an empty vessel which will only be filled by the determination (*Bestimmung*) given by the answerer: for example, "What is it?," "Where does it come from?" "When, where, why, and for what purpose did it happen?" The questions of discrimination (*Entscheidungsfragen*), on the other hand, already lay before the questioned person a possible judgment relationship (*Urteilsbeziehung*) and therefore in contrast to the questions of determination, can be answered by "yes" or "no": for example, "Was it a diamond?" "Was much damage done?" It is obvious that, on the average, in a considerable number of reactions, even though not in each individual case, the questions of discrimination must express a livelier mental activity than do the "empty" questions of determination. This idea is supported by the experiments which my book, *Das Seelenleben des Kindes,* also reports in a brief summary (p. 244). The proportion of such questions, which were not "empty," but, on the contrary, presented a judgment relation for decision, to the total of all questions was, in the material at my disposal, for pupils of 12-13 years only 2%, of 14-15 years 13%, of 15-16 years 12%, of 16-17 years 42%, and of university students 56.5%. An experiment conducted by A. Vetter in the *Realgymnasium* [junior and senior high school] in Darmstadt yielded similar results for the five successive classes *Quarta* to *Untersekunda* [about ninth to eleventh grade], but showed a sharp deviation in the preceding class, *Quinta* [about eighth grade]—a fact which showed the need for larger numbers of cases in such statistical calculations. I myself, however, have not been able to pursue this problem any further.[2]

In investigating interest in causal relations, P. Vogel, in an experiment, used not only the instigation to the asking of questions described so far, but also independent statements written down by pupils in response to definite stimulus words (lettercarrier, frog, glass, train, etc.). These statements yielded a very large number of thought relations (over 30,000 judgments).[3] It was shown by both methods that, when compared with other categorical relations, the category of causality stood out in the special frequency of its appearance and surpassed in this even the substantive relations. Here, however, I shall merely present the result of a trial test in which I myself attempted a slight improvement upon the method. In a seminar exercise in which 38 people took part I asked the students to underline, among the questions which they had already written down, those questions which they regarded as the most important to have answered. The percentage of causal questions was more than twice as large among the underlined questions as among the questions as a whole, where it was already considerable.

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[2] Elisabeth Kawohl was working with another problem when she found recently that in the child in its third year questions of discrimination appear in language somewhat earlier than do questions of determination. In any case, the author is here excluding the imitative questions and certain questions about names. (Die Kindliche Frage. *Erg. d. Vjsch. f. Päd.,* 1929.)

Although these findings cannot be connected with the problems of logic at all, and can only indirectly be related to epistemology, there is seen to be a somewhat closer connection as soon as one considers the question upon which the principal results of the previously mentioned experiments bear—namely, what is the situation, in the field of questions and judgments directed toward causal connections, with regard to the contrast between regressive thinking, which looks for causes, and progressive thinking, which looks for effects? Hume, as we know, reduces the consciousness of causal relations to habit. When similar occurrences have already repeatedly followed each other in experience, he says, we are conditioned to expecting, when an event reappears, that its usual accompaniment will also appear, and to believing that it will do so. This associative derivation of the consciousness of causality, from which Hume draws such important conclusions regarding the validity of our sources of knowledge, presupposes that, in the first place, causal thinking develops out of successions already several times repeated, and that, in the second place, it is progressively directed toward the future; for to Hume there seemed to be involved "prediction of future effect." In regard to the first presupposition, William Stern, in his *Psychologie der frühen Kindheit* (1st ed., 1914), has expressed doubts because in children and, similarly, in primitive peoples it is, on the contrary, under the impress of unusual constellations that interest in causal relations is activated—the impress of "new situations for which no expectations and habits whatever have yet been developed." My own experiments could be made to bear upon the second presupposition. They inevitably produced the impression that it is misleading, in determining the nature of causality, to regard progression as, so to speak, the basic phenomenon. Human thinking is at first directed with more energy toward the regressive path which leads back from what is given to its causes; that is, it likes to take a course in a direction contrary to the stream of the associations, up to the sources. The growing child is interested first in causes, as is revealed by its indefatigable "why?" The one-sided attempt to explain consciousness of causality out of the expectation of what is to come is therefore at least incomplete. This seemed to be shown also by the experiments in genetic psychology which I am discussing here. Specifically, it appeared that the progressive questions, i.e., those directed toward the later consequences of a situation, took up less space in the lower school classes than in the higher ones. From data consisting of 908 causal questions which I treated statistically (not all causal questions permit definite classification as regressive or progressive) it appeared that, if the regressive questions are divided by the progressive ones, we obtain for children 12-13 years old a quotient of 9.8; for 14-15 years, 7.4; for 15-16 years, 4.7; for 16-17 years, 3.9; and for university students 1.3. In the material of P. Vogel, also, though obtained in another way, the interest in progress was less in the lower age groups than in the higher. If these results, which, of course, I do not regard as final, should be further confirmed, we would gain, quite apart from the critical attitude toward Hume, an important insight into the development of the growing human being. In any case, the direction of attention toward the general problem of progressive and regressive thinking seems to me to merit consideration from several points of view. Thus, for example, the teacher should not overlook the question of whether the games, especially the fighting games, of children do not do more than does the whole of school instruction to favor practice in forward thinking, which is so important in the struggle for existence.[4]

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[3] Vogel, P. Untersuchungen über die Denkbeziehungen in den Urteilen des Schulkindes. (Giessen dissertation, 1911.) Leonard Oberer has now added to the work of Vogel, but had the pupils make verbal statements. (*Zsch. f. angew. Psychol.,* vol. 36.)

Later experimental investigations bore in part upon the problem of definitely directed imagination. These will be further discussed when the results are given in systematic order. There must also be mentioned the essay, published in Volume 9 of the *Zeitschrift für Aesthetik* on "Das anschauliche Vorstellen beim poetischen Gleichnis." This was based upon the experimental procedure introduced by Külpe, and often designated as the "quiz method" (*Ausfragemethode*), which introduces experimentally the experiences to be analyzed and attempts to gain knowledge of their characteristics through questions immediately following. Finally, there must be considered, as a late pendant, an experiment, designated merely as a trial test (*Stichprobe*), on the significance of "the voice of conscience." In this experiment a phenomenological problem belonging in the field of ethics was attacked, for the first time, to my knowledge, from the point of view of experimental psychology (*Zsch. f. Psychol.,* 1928). These experiments, too, will be further discussed later on.

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[4] For a utilization of this contrast in relation to typology see my book *Bismarck im eigenen Urteil,* 1920, pp. 243 ff.

The third line of work, the one most recently taken, is characterized by its object, but also again by the method adapted to this object. I had gained the conviction that scientific psychology should undertake not only the observation of living man, but also, more extensively and systematically than heretofore, the analysis of literary documents, usually fostered primarily by the historical disciplines. For, although such a "psychology of literature" is necessarily less important in many respects than direct observation of our fellow beings, it can, nevertheless, open up the way to problems which are attainable only with difficulty or not at all by other means. Thus, for example, it is not usually easy to carry out a deep-going analysis of the character traits of living associates, and insofar as it is successful, nevertheless, there are often enough external considerations which stand in the way of its utilization for science. One may perhaps be able to make very valuable observations on an acquaintance who stands out in his stubbornness, egoism, or vanity, but one would be in danger of unpleasantness if one published abroad the results obtained. There is also the interest of individual psychology in prominent personalities who have significance in the history of humanity. Among our own contemporaries such leading spirits are likewise present, but psychological observation of "the living object" is only rarely possible here, while the writings, diaries, and letters of the illustrious dead offer an enormous fund of material. And as to the psychological investigation of earlier stages in the development of the mental life of these people—especially since the psychological problems and the problems of historical research are not the same—for this we are entirely dependent upon the analysis of literary documents.

In studying such documents, the method of literary psychology must not rest satisfied with intuition and sympathetic insight (*Einfühlung*), although these basic means of understanding are indispensable and irreplaceable. Psychology as a science needs a working method which goes beyond subjective impression and is supported by an objective foundation susceptible to control. The first and most general requirement of a scientific literary psychology seems to me, therefore, to be directed toward collecting as complete a fund of material as possible in which the documentary evidence relating to the problem set has been prepared for analysis in a way which shows that its arrangement was governed by purely psychological considerations. After one is in possession of material thus arranged, it is to be recommended, especially when the need of comparison appears, that the data be analyzed by simple statistical methods also. This is a tiring business which should only be undertaken if the available material is sufficiently extensive and if there are not too many doubtful cases in regard to whose admission, or classification, at least, under specific rubrics, it is possible to have different opinions. (Quite comprehensibly, a psychological analysis has to struggle with this difficulty more often than would purely philological statistics on external speech-forms.) Nevertheless, literary psychology, wherever it seems at all worth while, should favor statistical evaluation more than it has done so far. As early as 1915, in publishing an essay on Fichte, still to be mentioned, I expressed my conception of the situation. The circumstances with regard to statistics in literary-psychological work seem to be similar to those connected with the first introduction of experiment into psychology. There is a tendency at the beginning, and perhaps a lasting one, to stay on the surface of the problems, and certainly often enough an effort is made for exact determinations even where the vague impression of a less time-consuming investigation would be sufficient. But working with figures does offer something irreplaceable, too. This applies above all, as already indicated, to comparisons, whether we are dealing with varying periods of time or whether several individuals from the same epoch or different phases of life in the same individual are to be considered. In such cases it is almost indispensable to give numerical relations. In addition, there is the fact that entirely new problems immediately appear as soon as we use the statistical method. The question, for example, of what the proportions are, in the language of a given author, for the expressions for bright colors, for the so-called "neutral" colors (white, gray, and black), and for "glittering, glowing, and shining" colors has perhaps only slight significance, but it would probably never be uttered at all if it could not be based upon a statistical calculation of the sense qualities utilized in poetry.

The question I have cited indicates the point at which my own first literary-psychological work began. Here, too, the choice of objects for analysis already showed a connection with the interests of aesthetics and the theory of art. In the so-called *Giessener Arbeiten* the primary aim was to work up the utilization of sense qualities (*Sinnesqualitäten*) in poetry by statistical methods. To the same group belong the essays written by me with the help of Ilse Netto and Marie Groos on "Die optischen Qualitäten in der Lyrik Schillers" (*Zsch. f. Aesth.,* 1909, vol. 4), "Ueber die akustischen Phänomene in der Lyrik Schillers" (*Zsch. f. Aesth.,* 1910, vol. 5), and also the "Psychologisch-statistische Untersuchungen über die visuellen Sinneseindrücke in Shakespeare's lyrischen und epischen Dichtungen" (*Englische Stud.,* 1911, vol. 43) and the essay "Die Sinnesdaten im Ring des Nibelungen" (*Arch. f. d. ges. Psychol.,* 1912, vol. 27). The dissertations of students, suggested by me and directed toward similar goals, have treated in part the poetic utilization of bodily movements and postures (W. Kostowa in regard to C. F. Meyer, J. Bathe in regard to H. v. Kleist), while others reached out beyond the sphere of poetry, Ch. Zimmermann comparing the description of visionary states in Suso and Saint Theresa, while M. Katz, studying the description of musical impressions by Schumann, Hoffmann, and Tieck, investigated so-called "colored audition" (synaesthesia), among other things (1912). Just here the advantage of numerical determinations showed itself again in the fact that the expression "colored audition" (*Farbenhören*) may lead to misunderstandings insofar as, in the three romanticists named, the mention of actual colors made up only 21% of the cases, while 51% of the references bore upon the phenomena of glitter, glow, and radiance. From other scientific laboratories, too, have come writings which start from the efforts here described. I will mention the essay by Kurt Kunze which appeared in Lamprecht's *Beiträge zur Kultur- und Universalgeschichte* in 1914 on "Die Dichtung Richard Dehmels als Ausdruck der Zeitseele," the Göttingen dissertation of Hans Trautmann on "Das visuelle und akustische Moment im mittelhochdeutschen Volksepos" (1917), and the dissertation written at Tübingen under the romanist J. Haas by W. Heege on "Die optischen und akustichen Sinnesdaten in Bernardin de Saint-Pierre's Paul et Virginie und Chateaubriand's Atala" (1917). In addition, I may also mention here C. Siegel's study of images and comparisons in Schopenhauer (*Zsch. f. angew. Psychol.,* 1927, vol. 29). Although this essay deals with another theme, it shows in its manner of execution a working method akin to my efforts and reveals again clearly the value of simple statistical determinations.

The need of testing the method on other themes was likewise a determining factor in the additional work on literary psychology which I carried out after the study of sensory qualities. Here I was placed before an alternative. I could either stay on the investigation of sensory qualities, develop further the possibilities for comparisons, and gradually improve the methods, which were still incomplete, or, in order to show the vast scope of literary-psychological problems, I could keep on attacking new tasks, threatening with new difficulties. It is probably in accordance with my inborn nature that I preferred the second path. All my publications, therefore, are to be regarded as groping steps forward which naturally still show many deficiencies but are meant to serve the purpose of encouraging further investigation in a field whose limits can as yet hardly be seen.

In 1913 there appeared in the *Zeitschrift für angewandte Psychologie* the essay "Der paradoxe Stil in Nietzsches Zarathustra." Here the immediate task was to examine the various types of paradoxical style in a concrete example, work which naturally led to the desire for comparison with other authors. From this need arose the dissertation of J. Wenter on "Die Parodoxie als Stilelement im Drama Hebbels" (Tübingen, 1914), which was carried out by a statistical method, a method which, for lack of time, I had omitted in the work on Nietzsche, to my subsequent regret. Apart from the aesthetic problem, unpsychological in and for itself, the analysis of paradoxical style offers also the possibility of penetrating more deeply into the nature of a personality than is feasible in working out the sensory qualities, which, in the main, leads only to conjectures regarding the reproductive type of the author (for example, the strong acoustic imagination of Schiller, the motor imagery of Kleist, etc.). The study of Nietzsche brought forth also many other questions, leading beyond his work, which might stimulate further investigation. Among these I count the relation of paradox to the language of religion, especially the language of the mystics, who, it seems to me, like to use paradoxical turns of speech because paradox, meaningful despite its superficial contradiction, is the suitable means of expression for a way of thinking which permits the irrational to triumph over the rational. The statements to be found in my essay which refer to the penetration of paradox into simile could also lead to further investigation. There is still much to be done here.

If the paradoxical style is found only in people of a special type, there may be another question by which we may be able to attack the great problem of personality much more extensively: ought not the collection and analysis of statistical data on the judgments of value uttered by an important personality be able to yield significant services to the investigation of character? Precisely in this matter the observations already made on the possibilities in literary-psychological statistics seem to me to be on the right track. The method may be called external and superficial, and it may be repellent in its dryness, but it does bring differences to light which without it would either not be observed at all or else not determined with sufficient definiteness. If, for example, we gather the judgments of value expressed by a man about people, questions such as the following are raised and answered: Does he like to make judgments about himself or is his gaze directed rather toward the value of his fellow beings? Are his judgments predominantly laudatory or predominantly critical? What sort of a shift takes place in the statistical proportion between positive and negative valuations if we pass from the evaluation of the environment to self-evaluation? Does discussion deal more often with the intellectual or with the ethical qualities of people? What differences appear between the different periods of the man's life? Are they related to his external experiences? The answering of such and similar questions seemed to me to present an objective basis for characterology which should not be undervalued. As an experiment, I asked the city vicar, G. Josenhans, to work out the judgments of value in Fichte's letters and diaries. His dissertation remained, alas, unfinished, for he fell in the World War. On the basis of his notes, however, which were already very extensive, I published the statistical findings in the *Zeitschrift für Philosophie* in 1915. A parallel investigation was then presented by Max Sting, who, in a dissertation of 1919, worked out by the same method the judgments of value appearing in Schopenhauer's letters. "Judgments of value," he says (p. 3), "are like windows through which we may look into the inner life of a person."

My later contributions to literary psychology dispensed (partly for external reasons) with statistical determinations. The book which appeared in 1920 on *Bismarck im eigenen Urteil* continued the distinction between evaluations of the self and evaluations of the environment which had been carried out in the work on Fichte, but expanded the category of self-evaluation (*Selbstbewertung*) into the concept of self-judgment (*Selbstbeurteilung*), which included also reflections on one's own nature which could not be regarded as actual judgments of value. This explains the title of the work. Naturally, the investigation was not based exclusively upon self-judgments, although these are strikingly numerous in Bismarck, but also took into consideration the testimony of his contemporaries about his personality. The presentation developing out of the material again revealed clearly the biological orientation of the author's thinking. For I did not attempt to deduce Bismarck's nature out of his relation to objective cultural values (in accordance with Springer's procedure), but started from the innate instincts, among which the instinct of pugnacity stood out dominantly. If I had to write the book over again, I would probably try to combine the two methods of approach, but in such a way that the individual stamp of the instincts would still hold as one of the deepest roots of individual uniqueness. The work on Bismarck was followed in 1922 by the book *Fürst Metternich: eine Studie zur Psychologie der Eitelkeit.*" Here it was less a question of self-judgments than of collecting the numerous expressions from which an impression of the unusually strongly developed self-consciousness of the famous statesman could be gained. The object of the work was to present as clearly as possible, in a concrete individual case, the varied effects of vanity; for I think the psychiatrist Bumke was probably right when he said, at the scientific congress in Königsberg in 1930, that it was only upon the single individual that psychology's task of understanding the human being as a whole could be fulfilled with any certainty. The orientation of the work was purely psychological. It could not and did not intend to offer any new facts to the historian and it was quite free from any need of an ethical evaluation of the personality it was studying. If my powers suffice, I would like to carry out still other studies of the type last described. Temporarily, it is true, I am still tied to tasks which do not belong to psychology.

My book on the *Aufbau der Systeme* (1924) will be mentioned here only incidentally, since, although it can stimulate psychological questions on every hand, it is nevertheless unpsychological in its basic attitude, as the systems in it are regarded as independent structures separate from the thinking subject.[5] This is shown also in the fact that of the essays on the same subject, preparatory to the book—essays which appeared in the *Zeitschrift für Psychologie* from 1908 to 1917—I omitted from the book edition the fairly detailed essay "Zur Psychologie der Entgegensetzung" (1910). For this very reason the latter may well be mentioned here as a contribution to the psychology of thinking. In addition, there are three further publications belonging in the field of literary psychology—on "subjective visual perceptions" (*subjective Anschauungsbilder*) in Goethe, Spinoza, and Jack London—which I shall mention in the second part of my exposition.

At the conclusion of this historical section of my essay let me mention furthermore a literary-psychological dissertation by Hermann Haufler, suggested by me, on "Kunstformen des feuilletonistischen Styls" (1928), and also two studies which, it is true, only work up written material, but which in their method are related to the attempts here described. The first and smaller one is a dissertation by Walter Ludwig which appeared in *Beiheft* No. 21 of the *Zeitschrift für angewandte Psychologie* under the title "Beiträge zur Psychologie der Furcht im Kriege." The analysis of written observations which was made in this essay gave some suggestions for a very extensive dissertation on criminal psychology with which Walter Luz obtained his degree while with me. This dissertation was then published in two separate books: *Das Verbrechen in der Darstellung des Verbrechers* (*2. Beiheft der Monatsschr. f. Kriminalpsychol. u. Strafrechtsref.*) and *Ursachen und Bekämpfung des Verbrechens im Urteil des Verbrechers* (Heidelberg, 1928).

II

In the second part of this exposition I shall present a survey, arranged more according to systematic points of view, of the convictions which I have represented as a psychologist. They have been expressed for the most part, though not completely, in my *Seelenleben des Kindes,* a book which reaches beyond its actual theme and contains many general psychological expositions. In many respects, it it true, my views have changed. I shall take account of this fact in what follows.

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[5] The *Psychologische Anmerkungen zu Kant's Phenomenalismus* (Die Akademie, Erlangen, 1925) I likewise mention only in this statement, since the psychological interpretation here serves rather to explain the theory.

I shall begin, as is proper, with the problem of method. In the book mentioned, I have sketched a general classification of psychological working methods which has clearly been influenced by the biological interest stressed by C. Siegel. I state there (p. 11) that the problem of method is to study the working technique to be developed by research. The three phases of the complete reaction scheme—reception of the stimulus, inner elaboration of the perceived material, and motor discharge—run through the activity of the psychologist as they do through every other activity. Correspondingly, it is possible, I said, to distinguish in psychology three main groups of methods: the methods of observation, the logical elaboration of what is observed, and the external presentation of what has been so elaborated. The methods of observation were discussed rather fully in the *Seelenleben des Kindes,* upon the basis of a division according to three pairs of contrasts, namely, between self-observation and observation of others, between observation on the individual and observation on the group, and between observation under natural versus artificial conditions. In relation to each antithesis, the possibility of a mediation or connection between the contrasting members was also considered.

In regard to the methods of elaboration and presentation, which I have not discussed further in my writings, I would like to take the opportunity offered here to insert at least a few remarks concerning the contrast between construction (*Aufbauen*) from parts and deduction (*Ableiten*) from the whole—remarks which in part take a critical attitude toward some earlier statements. It is well known that in Germany, as elsewhere, a strong reaction has set in against the "mosaic" or "building-block" point of view, according to which the data of mental life are to be built up out of elementary contents of consciousness. It may well be said that this manner of building up a structure out of elements completely misses the special quality of mental life as soon as it attempts to express actual events. For the concept of psychic components which, as elements, have been "there from the beginning" (*"zuerst da sind"*) and from which then everything which we experience is built up "like a wall out of bricks" is, of course, fundamentally twisted. Nevertheless, we read, in the writings of even so important a psychologist as Brentano, the following definition of his descriptive psychology or "psychognosia": "it shows," he says, "all the ultimate psychic components from whose combination result all psychic manifestations as all words result from letters."[6] Are we seriously meant to think here of real psychic constitutents and of their real combinations? Many psychologists who have made use of this terminology have explicitly pointed out that the concept of a structure built up from ultimate psychic constitutents was by no means meant to be taken literally; I myself indicated this in my *Seelenleben des Kindes.* In that case, in connection with our classification of methods, the point of view described could be defended as follows: The discussion of parts and their combinations is by no means intended to represent the result of observation; but there are also methods of elaboration and presentation, as we saw. In order to be able to elaborate more accurately what is present in observation, we must analyze the totality of mental life into separate parts (by a pure *distinctio rationis*), and, in order to gather together what we have found through isolation, we use the simile of combination. Neither process is intended to be a description of actual conditions; it is to be regarded merely as a useful fiction.

This seems reasonable. But, on closer inspection, opposition still arises. Why does the author choose for his purposes expressions which may so easily lead the reader astray, and which, one may add, expose even the author himself to the danger of sometimes confusing fiction and truth? I myself, in my book on child psychology, spoke of mental "material" and its "syntheses," distinguishing between linkings (*Verknüpfungen*) and interpenetrations (*Verwachsungen*). I did, it is true, emphasize the idea (p. 30) that we were here dealing with an artificial analysis (and synthesis) of the unitary mental life which was possible only in abstraction. But would it not be more sensible to keep away as far as possible from the mosaic point of view even in terminology, and to use, for the purposes of separation and connection which indubitably exist, expressions which are less likely to lead to misunderstandings? To my mind the concepts of "elements," "parts," and perhaps also that of "mental material," are the most risky, because they originate in the concept of material bases remaining constant and probably also primary and do not allow the idea of the fundamental unity of the subject (*des Subjekts*) to emerge properly. Psychology with a biological orientation proceeds differently. If it starts from the psychophysical reactions of the animated organism, as I did in my contributions to the study of play, aesthetic behavior, and personality, it will, it is true, likewise have to turn itself to isolating abstractions in its elaboration and presentation. Still, however, it is not starting from ultimate persistent elements, but from shifting processes which do not deny their origin in the life-unity of the individual. We should probably speak in similar fashion elsewhere also of various processes, conditions, and functions which proceed from life-unity and continue to reveal this origin even when they have been given independent status in a rational distinction.

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[6] In his *Letzten Wünsche für Oesterreich,* written in 1859. Cf. the introduction by Oskar Kraus on p. xvii of *Psychologie vom empirischen Standpunkt,* recently issued as one of Felix Meiner's *Philosophische Bibliothek.*

When the elements have thus been replaced by partial processes, part functions, etc.,[7] the concept of combination, insofar as it does not relate exclusively to the method of presentation, will likewise have to be so used that a closer correspondence to mental reality may appear. But such a correspondence actually exists, for we may stress the idea that life cannot be explained wholly on the basis of influences which come down vertically, as it were, from the unity of the whole, but that vital occurrence is characterized also by a "horizontal" interaction between its partial processes (*'Teilprozessen'*).[8] This interaction, it is true, takes place only within the whole and is continually subordinate to the latter's guidance, but it would be narrow to overlook it. When I wash my hands I am acting under the guidance of the willing ego, but the fact that the right hand washes the left and vice versa also belongs to the whole process. Perhaps what I mean may be made clear through a social example. A folk song would certainly not be possible without the influences emanating from the people as a whole; but the correct words, which belong to the whole, are nevertheless always found by an individual person who influences other individuals by them. Here, in order to do justice to the process, we need a "palintropy" of methods.[9] To deduce the folk song directly from the totality would be just as narrow as to explain it exclusively from the interaction of completely isolated individual minds. Such a deduction would remain one-sided even if it were stipulated that by folk soul was meant a really existing super-individual group personality (*Gesamtpersönlichkeit*). What the zoologist Spemann has said about "organizers" (*Organisatoren*) applies in this field also. The situation with regard to the partial processes in the life-unity seems to be similar. They are present only in its totality, but also interact upon each other as psychophysical processes. From this we may probably explain what remains justifiable in the theory of associations, for example. The conceptual processes (*Vorstellungsprozesse*) and their physiological bases are not independent entities, but partial processes (*Teilprozesse*) in the totality of the individual, but in order to understand their succession it is probably necessary, nevertheless, to regard the individual process as a potent factor which is used by the whole in making a transition to another partial process.

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[7] My colleague Haering once said to me in conversation that in his opinion many of the so-called "fictions" can be avoided through the use of suitable expressions.

[8] Cf. here W. Stern's *Person und Sache.*

[9] My last discussion of palintropy of methods was in the little essay "Methodik und Metaphysik" (Tübingen; Osiander, 1928).

After these methodological considerations, I pass over to the problem of subdividing the mental life. Classifications of subject-matter are almost always disputed, since there is rarely agreement regarding the choice between the possible bases for analysis. In psychology, additional difficulty is created by the fact that much in our experience is too fleeting and vague to be held fast in introspection, and that, in addition, the concept of soul (*Geist*), if we are to mean by this a real life-unity, extends beyond such vague experiences far into what is unknown to us. We may question in advance, therefore, whether complete classification will ever be possible in psychology. In any case, if we agree with the previous considerations, we must request that the analysis speak not of persistent and independent parts, but of occurrences or processes which arise from living unity and arrive at interaction only in this unity. The spiritual living-unit (*seelische Lebenseinheit*) itself (the *Entelechie* of Aristotle and Driesch) is more comprehensive than consciousness, but makes itself felt in the latter (at least in man) as that which we call ego or self. The processes will then be traced back to forces, powers, or, as we prefer to say now, "dispositions," and the expressions of dispositions be traced back to "acts" of the soul (*Akte der Seele*) and the processes themselves will be designated as "states" of the soul. As a point of departure for the classification, we may choose the dispositions, or the acts, or the states. The dispositions are entirely unconscious; the acts as such, if we mean here the activation of the dispositions, penetrate only in part into the sphere of consciousness (in the tension of attention and in the fiat of volition).[10] The states, too, are only partially experienced, if we regard the soul as a real life-unit.

If, as I should like to do here, we start from the acts of the soul, we can here distinguish between acts of representation (*Vergegenständlichung*) and acts of evaluation (*des Wertens*). The acts of representation make states possible which consist either of intuitive or of non-intuitive representation (*anschauliche oder unanschauliche Vergegenwärtigung*) of "something." The scale of the intuitive extends from sensory perceptions through hallucinations and subjective representations up to the palest reproductions. Non-intuitive states are those of "consciousness" (*Bewusstheit*) (N. Ach): awareness of relations, meanings, contexts, imports, values. The acts of evaluation (here I am going on from my former publications) are revealed in the triple polarity of the attitude assumed by the subject, namely, as an emotional evaluation in terms of pleasantness and unpleasantness, as a volitional one in striving and resistance, and as an intellectual one in acceptance and rejection. The acts of evaluation presuppose intuitive objectiveness as a basis and make possible on their side the sphere of non-intuitive objectivizations; they may then, however, subject these again to scrutiny from the standpoint of value. The classification indicated here probably needs correction in many respects; fundamentally, however, it seems to me to arise from a conception which will be heard of in the future. The relation of the psychic to the physiological I have purposely left indeterminate in this connection. I was led to introduce the idea of a three-fold polarity of value through reflection upon the James-Lange theory, because I was convinced, on the one hand, that this theory was of essential significance with regard to the intuitive side of our emotions and voluntary impulses, but, on the other hand, found that, in it, the subject and his attitude received too little consideration. Subsequently, I noticed that the distinction between a representational and a valuational side in our mental life is rather similarly made by Brentano and, with still closer similarity, by Heinrich Rickert. That investigators as different as Brentano and Rickert should converge toward this point seems to me to speak for the justification of the theory. More about it can be found in the *Seelenleben des Kindes*. All three types of acts of evaluation are involved in the objectivization of values, but in my opinion affection (*Gefühl*) and volition are of decisive importance here. Therefore, in contrast to Rickert (whose whole system depends upon the answer to this question), I would not, considering the sense of judgment as non-volitional, regard the latter without hesitation as a value.

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[10] If we wished to limit ourselves solely to what is experienced, the reflections which I made (*loc. cit.*, p. 23) against the general use of the word "act" would probably be justified.

I now continue my survey. In regard to the theory of sensation, I must state first that in my book *Die Spiele der Menschen* I dealt rather explicitly with the pleasure of experiencing sensory stimuli. In this connection I emphasized, among other things, the idea that not only the soft, insinuating stimuli, but also strong stimuli, sometimes even accompanied by unpleasantness (*Unlust*), may be a source of enjoyment, a fact which likewise can only be explained from the point of view of the whole (*vom Ganzen aus*), according to which the strong stimulus, perhaps unpleasant in itself, sends a high wave of life through the organism, arousing feelings of pleasure in the latter. What can here be established outside of the realm of art (for instance in regard to mild and sharp gustatory stimuli, or gentle and violent motion) is repeated upon the heights of aesthetic experience and reveals itself here in the distinctions which lead from antiquity, through Burke and Kant, to Schiller's contrast between the languishing and the energetic. The reproach which is sometimes made against such comparisons, that they are sensualistic, appears invalid as soon as it becomes clear that, in the first place, comparison does not necessarily signify identification, and that, furthermore, for example, in regard to the pleasure in taking a cold bath, the entire living-unit (*Lebenseinheit*) may be involved, even in its moral feeling. My interest in the already-mentioned James-Lange theory is likewise related to the psychology of aesthetic experience—the theory which holds that the facts of coenaesthesis form the nucleus of affective and volitional experiences. It is in the essay on "Das esthetische Miterleben und die Empfindungen aus dem Körperinneren" that I discussed this most fully. I shall return to it later. The literary-psychological writings on the evaluation of sensory qualities in poetry may also be mentioned in this connection, although the artist is here concerned rather with the reproduction of what has been previously perceived. In these studies, various visual qualities were noticed which usually receive only slight attention in the textbooks but which are very important for literary presentation. This applies particularly to glitter and glow. Finally, I shall mention also the little essay on "Lichterscheinungen bei Erdbeben" (*Zsch. f. Psychol.,* 1913, vol.

65), in which I tried to show, on the basis of introspections, that such phenomena, when the natural event occurs at night, may, under some circumstances be solely subjective, since twitching movements of the rested eye occurring during fright may induce lightning-like impressions of light.

I come now to the great realm which is usually called the field of reproductions. I would like to state first that, in contrast to the usual division into images of recall and images of phantasy, I have proposed, for the experiences considered here, a three-fold division according to which we must distinguish the "free imaginations," which represent a simple experiencing of internal images (for instance, the image of a white cloud when we read the words for it) from "past" images (*Vergangenheitsbilder*), on the one hand, and "future" images (*Zukunftsbilder*), on the other, in both of which there appears in consciousness not only the pure representation but also the temporal tendency (*Intention*) toward past or future material (*Seelenleben des Kindes,* p. 30). This distinction, which has also been taken over by C. Siegel, seems to me to be necessary to genetic psychology. All internal images, objectively considered, naturally presuppose previous experiences. This applies to the free imaginations as well as to the images which contain an expectation of the future. But the conscious temporal tendency is something different. It may even be considered not improbable that, in the early development of the child, images of the future may appear before the images of the past or at least obtain practical significance before the latter. Here a marvelous adaptation is revealed, as I then explained (*loc. cit.,* pp. 259 ff.). The internal image possesses biological utility[12] only when similar combinations are repeated in the reality independent of the ego, from similar causes and in similar fashion. For it is only if similar material recurs in the future that the reproduction of past experiences acquires significance for the conduct of life. The renewal of earlier experiences can then be used for anticipating what is to come, either as a warning or as a lure. In the little essay published by E. Diedrichs in 1909, "Die Befreiungen der Seele," I expressed this as follows: "Before it can learn to look backward, the soul needs the Promethian gift of being able to see beforehand in the magic mirror the coming thunderstorms and rainbows. It is the special skill of memory that, looking toward the return of everything lawful, it summons from the grave of the no-more the images of the yet-to-be."

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[12] In the third edition of my *Spiele der Tiere* (pp. 48 ff.) I proposed that so long as they only establish actual life value without reducing it to teleological principles, we should call such observations not teleological but "opheletic" (from ὠφελεῖν, to be useful).

I have made a few contributions to the theory of the "subjective representations" (*subjectiven Anschauungsbildern*) which the so-called "eidetics" have the power to summon. Even before these manifestations had become familiar in wider circles through the studies of E. Jaensch and O. Kroh, I had come into touch with the problem of hallucinoid images, as is shown in the thirteenth section of my book on child psychology and in the essay "Zur Psychologie des Mythos" (*Inter. Monatsschr.,* 1914). When Kroh's publication on *Eidetiker unter Deutschen Dichtern* then appeared, I attempted, in the little essay on "Goethe als Eidetiker" (*Die Umschau,* 1921), to complete the material presented by Kroh with a few additional references, by one of which, a description from "Wilhelm Meister," I was led to point out the existence of a vision of an aura (cf. Rudolf Steiner). To this was added a notice appearing in the *Bremer Nachrichten* (July 8, 1924), in which I called attention to a remarkable narrative in Spinoza's letters which may lead to the question whether this philosopher likewise experienced halucinoid images. A fuller contribution in the same field of problems is the essay "Die Verwertung der Eidetik als Kunstmittel in Jack London's Roman Martin Eden" (*Zsch. f. angew. Psychol.,* 1929). The hero of this novel, in whose nature and fate much of the author's own personality is expressed, is a pronounced eidetiker. While pointing out this fact, I raised a question which, to my knowledge, has not been very fully treated as yet in the literature on eidetic imagery, namely, in what manner is the appearance of subjective visual representations utilized by the eidetic poet as an artistic tool (for example, for purposes of characterization or exposition)? I think that it would be worth while to pursue further some of the problems raised in this connection.

The next point which I would like to touch upon concerns the theory of associations. As soon as the conditions involved are considered with any care it must be apparent that particularly in this situation the "building-block" point of view is out of place, although historically it stands in especially close relation to the theory of association. If, in response to the same word, Locarno, now the Lago Maggiore, another time the name of Stresemann, and, in a third case (because we are looking for a rhyme), the River Arno comes to one's mind, it must be more than the word symbol, in fact, it must be the whole mental constitution which induces now one and now another response. It is a demonstration of the great power of methodological assumptions and habits that (at least in verbal expression) the process has always been presented as though the individual idea as such drew another individual idea after it. Even in my own book on child psychology the section on associations has been held too close to the old form of presentation, although in the discussion about them the way was opened up for a more pertinent conception. Naturally, however, there must, nevertheless, be something true in the concept of successions of ideas conditioned by association. I am thinking here particularly of the effect of contiguity, of linking in previous experience. The signs heard or read fit themselves into a sensible context to which they must already have belonged; otherwise orientation (*Einstellung*) would not result. In most cases, as in our example, tendencies (*Dispositionen*) toward various connections are present, and the decision in favor of one or another depends upon conditions in the mind at the time. In recalling the Italian river name, too, the process, in my opinion, is that the sound symbols "Arno," which at first belong to the complex "Locarno," realize for us, as though in a jump, the context "river near Florence," familiar to us from previous experience. To assume a special "association by similarity" seems to me, as I tried to show in the book I have mentioned, not to be necessary anywhere, and hence not here either. On the other hand, as in all lawfulness of occurrence, the concept of similarity does always play a rôle in the sense that there is never absolutely identical recurrence. It is not the same but only a similar happening which bobs up again and induces the mind to enter upon processes which, likewise, are merely similar to those which were formerly in connection with that happening. The rôle of similarity here, I said (*loc. cit.*, p. 92) is different from what it is in so-called association by similarity. Let us look at the accompanying schematic representation. We may then express the situation as follows: In real association by similarity the temporal connection between a^1 and b^1 would be unnecessary; a^2 would lead directly to a b^2 similar to it. According to our view, the horizontal connection between a^1 and b^1 would have had to come first. On the other hand, the vertical connections in every association contain the relation of similarity between a^1 and a^2 and between b^1 and b^2.

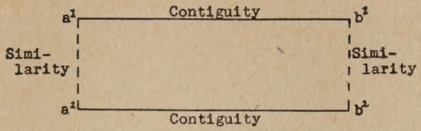

FIGURE 1

August Messer, who likewise represents this view, has formulated it as follows in his psychology: "Thus, strictly speaking, it is only the law of contiguity that is really a law of association, because it furnishes the conditions for the formation of connections between bases of reproduction (*Reproduktionsgrundlagen*). The law of similarity, on the other hand, is not a law of association but a law of reproduction, since it states that the foundations of reproduction are actualized, i.e., stimulated to the renewal of previous experiences, not only by the same[14] but also by similar states of consciousness."

The management of a difficulty connected with my limitation of association to association by contiguity shows clearly that even then I was trying to get beyond the building-block point of view. For, if we consider poetic simile—for example, the idea of rose petals which comes to the mind of the poet at the sight of the cheeks of a young girl (we are assuming that the comparison is not borrowed but original)—it is naturally not necessary that girls' cheeks and rose petals should already have been perceived together in previous experience. How can we, nevertheless, carry out the scheme of association by contiguity? One might be satisfied with the explanation that it was the concordant color which now appeared in the complex "girl's cheek" but had previously been experienced in the complex "rose petal." But this did not seem to me to be sufficient. The temporal mediation, I said (*loc. cit.,* p. 90), is made possible in such cases through a "total state" of consciousness; (here the view which considers the "whole" made itself felt). The sight of the delicate girlish cheek fits into the emotionally colored impression "of something fresh and blooming which is pleasant to look at and soft to feel." The receptive poet has experienced a similar emotionally colored state at the sight of a new-blown rose, and the occurrence can thus be explained through "indirect contiguity." In connection with this explanation, I made an observation which I would like to repeat because it seems to me to offer a contribution to the understanding of poetic creation. "If the view presented here is correct, we also understand why all sorts of similes and metaphors come to the poet's mind which the simple average person would hardly happen upon. The fact is that the poet has unusually strong emotions in connection with his experiences[15] . . . . and gains, through the intense excitement, the capacity for startling associations based upon indirect contiguity" (*loc. cit.,* p. 93).

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[14] Strictly speaking, however, no two occurrences are ever completely alike.

In the year 1924 I published in the *Zeitschrift für Psychologie* an essay, "Ueber wissenschaftliche Einfälle," which, as the superscription states, pertains to the theory of thinking. Nevertheless, I am mentioning it here already because in connection with it I emphasized strongly the importance of associative bonds. The problem was the sudden emergence of intellectual relations as I was able to observe it in myself, particularly in states of insomnia, and in a few cases to analyze it immediately after the experience. For such associations, which are often startling in their appearance, what I said in discussing psychological methods (pp. 24 ff.) seems to be pertinent, namely, that it is possible to regard mental part-processes as potent factors which have at least a relative independence within the living whole, though emphasis must be laid upon the modifier "relative." In order to save space, I will choose as an example the sudden idea (*Einfall*) —not a scientific one, it is true—which I mentioned before—the bobbing up of the river name, Arno, which followed upon the word "Locarno." Only, in order that the process may be used for illustrative purposes, we must remove the assumption that there is the conscious intention of looking for a rhyme for Locarno; for here we are concerned precisely with conditions under which the guiding conscious unity (*Bewusstseinseinheit*) recedes into the background. We assume, therefore, that a man who comes to speak of Locarno in a piece of rhymed poetry feels himself compelled, as though by foreign forces, quite without conscious search for a rhyme, to a continuation of his poetic expressions, in which Florence and the Arno play a part. In this form, too, the process is possible only in the unity of the mind, and not comprehensible without a subconscious "set" (*Einstellung*) toward rhymes. But it is understandable that the part-process "Locarno" must here appear almost like an independent force which propels the part-process "Arno" out of itself, the unity of the mental being not appearing clearly in connection with the process. Speaking more precisely, the letter combination "Arno" which, just before, still belonged to the context embodied in "Locarno," suddenly and unintentionally plunges into the new context without the revealment of any unity in consciousness guiding the occurrence. The guidance is present, but it remains, as it were, behind the scenes. I tried to show in the essay I have mentioned that similar diagonal jumps are characteristic of sudden scientific ideas also. The essay also contains a few observations upon various types of experience which can be made in regard to such sudden ideas. I hope that other psychologists will develop this theme still further; perhaps a questionnaire might also yield useful material.

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[15] Better, emotionally colored total states (*Gesamtzustände*) which are made possible by that persistence in the gazing state of pure observation which is practiced more frequently by the poet than by the man in practical life.

Another fact in explaining which I likewise tried to maintain the relation to association by contiguity is the matter of "imagery with definite trend" (*bestimmt gerichtetes Vorstellen*). If we regard the successive expressions of reproduction too much as isolated components of the mental life it is not possible to understand why the course of imagination is able to maintain a definite direction or at least to return to this direction after a deviation. For every part-process in imagination contains points of attachment to the most varied associations, so that our thinking would be reduced to a confusion akin to flight of ideas if guiding influences did not compel us to stick to the point. It is known that H. J. Watt and N. Ach in their experiments, published in 1905, on the effect of "projects" (*Aufgaben*) and "determining tendencies" were thinking primarily of guidance by the will, as Hobbes had thought already when he said that the train of thought was of two kinds, one wild and disordered in the wandering of its images and the other regulated by a wish and purpose. On the other hand, it had been shown by W. Schäfer (1904), in a Giessen dissertation, instigated by me and entitled *Ueber die Nachwirkung der Vorstellungen,* that a disordered course of the images is opposed also by the influence of emotions and interest, including their unconscious or subconscious effects. I therefore assumed that all rather strong excitements involving the whole living-unit, through their after-effect which Otto Gross has called *Sekundärfunction* and which extends far beyond what directly follows, must favor imagination that has a definite trend. For such states, found at the beginning of the process (not only wishes and resolves but also emotions and intellectual interests), consciously or unconsciously persist and exert their influence toward preventing entrance upon by-paths which are not connected with the context originally present. The idea that we must not think here of separate, initial images, but rather of the total state of the animated being was brought out in the later editions of my book, in connection with Otto Selz.

My thinking about imagery with definite trend was stimulated also by a problem expounded by Hume, who pointed out as very striking the fact that when we hear a false judgment we generally think at once of examples which tend to refute the incorrect assertion. I cannot here discuss in detail the experiments conducted in relation to this (cf. *loc. cit.,* pp. 97 ff.). On the other hand, it will be necessary to add a few words regarding the manifestations which I have called *Erlebnissphären.* The after-effect of an attitude (*Auffassung*), intention, interest, or emotion, naturally determines not only the course of the imagery but also our entire behavior and state. It may give to an extended series of the most varied activities and states a common color, a total character, which is soon restored even after superficial disturbances. If, however, it finally yields to another attitude, a new mental context begins which likewise forms such a unity. In the school child the play sphere is set off from the work sphere in this way, as a self-contained succession of experiences whose character remains determined by the initial facts of voluntary beginning, enjoyment of social association, and expectation of pleasure even when these are still only undercurrents in consciousness. In the existence of the adult there likewise arise clearly separated spheres of experience, so that even in the realm of the normal we might gain the (erroneous) impression that in the daily life of the same individual quite different sorts of personalities succeed each other. A man who, in relation to his chiefs, is ruled by the spirit of obsequiousness may be an irritable tyrant at home and a fairly affable comrade at the bowling club or over a glass of beer in the evening. With entrance into the new sphere there takes place a shift in disposition which dominates the person's entire state and behavior and actually almost blots out the recollection of the sphere immediately preceding (although this recollection always remains possible for the normal person), while, on the other hand, the threads of the corresponding earlier experiential connections are resumed as though they had never been interrupted.[16]

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[16] *Das Seelenleben des Kindes,* pp. 111 ff., 58, 77, 151, 182. Cf. *Das Spiel, zwei Vorträge,* 1922, pp. 16 ff.

Recently Kohnstamm (Amsterdam) used these explanations in connection with the definition of the concept of play.

The experimental contributions to the psychology of thinking have already been mentioned. Now, since there are still a few things which need to be stated here regarding the activity of the intellect, I shall begin with a train of thought which is connected with these experiments. In discussing them, I introduced (pp. 5 ff.) the distinction between questions of determination and questions of discrimination ("Who was it?" *vs.* "Was it perhaps my brother?"). The former are, as it were, empty vessels which are to be filled by the answerer; the latter can be answered by "yes" or "no," because they themselves already propose a judgment relationship (*Urteilsbeziehung*), which requires only confirmation or rejection. If for the answerer we substitute experience, and if for the articulate question directed to another person we substitute the "inner question," i.e., the state of intellectual uncertainty which looks toward the acquisition of knowledge and which seeks to become a state of certainty, we hit upon what seem to me to be the most fundamental motives for the development of knowledge which are related to this grammatical distinction: surprise at the unknown and conscious expectation of what is to come. The treatment of these two motives runs through the whole sixteenth section of my *Seelenleben des Kindes,* but is perhaps too much obscured by various interpolated explanations. The inner question of determination arising from mere surprise at what is unaccustomed can be transposed into knowledge only through the grace of experience. The person, however, who transforms the inner question, empty as yet, into a question of discrimination which he has filled with his own conjectures and expectations, can force experience to answer even when it is mute. I have tried to show (*loc. cit.,* pp. 239 ff.) that this active acquisition of knowledge, culminating in the verification of hypotheses, corresponds to the type of conclusion which is designated in logic as "mixed-hypothetical" but in which, in the actual thinking process, the path of reasoning from conclusion to premise, forbidden in logic, is often taken (if A is, then B is; now B is, therefore A is).

The independent acquisition of knowledge thus characterized is, of course, to be contrasted with the adoption of information from the outside, through teaching. In this connection (*loc. cit.,* pp. 230 ff.), in regard to the learning child, I have distinguished several stages of acquisition which seem to me to be involved in the psychology of adults also. The lowest stage, it is true, will rarely be found in the latter, we hope—the stage of completely uncomprehending absorption of the mere verbal sounds into the memory such as is shown by the four-year-old child reciting a poem in which occur the words "six times six is thirty-six." A stage above this is understanding the meaning of the transmitted word, which may be characterized in German as *Bewusstheit* of its meaning. From mere understanding of a judgment is further to be distinguished the recognition of its correctness. This recognition may be based upon simple "acceptance in good faith" which is rooted in confidence in other people's truthfulness but also sometimes in the suggestive power of conviction radiating from a person. Or it may appear only conditionally as an "assumption" (*Annahme*) (in Meinong's sense), where one simply presupposes that things are as the judgment states them to be. Or it may arise as actual consciousness of the validity (*Geltungsbewusstheit*) of one's own insight into the truth of the transmitted material, whether this insight is gained through direct evidence or whether it rests upon deductive derivation from premises or upon inductive confirmation through experience. Naturally, ideally speaking, all transmitted knowledge ought to be transmuted into this personal insight. But, practically, this is possible only to a relatively limited extent. Thus our knowledge of natural science and still more of history depends to an almost appalling degree upon acceptance in good faith; and the same thing can probably be said about the convictions of those who follow with passionate agreement the oratorical arts of a politician.

The two studies mentioned in the beginning, *Ueberzeugung und Wahrheit* and *Die Sicherung der Erkenntnis,* go back to a conception which I first sketched in an article on aesthetics in the *Kuno-Fischer-Festschrift* (1904 and 1907) and which in its main lines corresponds to, although it does not quite coincide with, the independently executed expositions of Hugo Dingler and Del Negro. The line of thought which governs the whole belongs to epistemology and will therefore not be discussed here in detail. Nevertheless, the superscription *Ueberzeugung und Wahrheit* does point to the psychological side of the exposition. From the genetic point of view, I maintain that what, objectivizing, we call a truth is a projection of subjectively experienced convictions into the transsubjective, much as "evidence" is a projection of the experience of evidence. The resultant conclusion, that on a purely theoretical basis we cannot refute relativism in epistemology but must resort to "primacy of the will" if we wish to retain our faith in our ability to attain truth, no longer belongs in psychology.

The following remarks relate to the sphere of reactions connected with the affective and volitional life. As a biological psychologist I approached them predominantly through the instincts. As has already been brought out, these reactions can only be understood through the living whole, which explains why the representatives of the mosaic psychology generally did not wish to have much to do with them. Furthermore, they are also difficult of approach from the standpoint of mental elements because these penetrate only partly into the field of conscious experience. For discussion of my conception of instincts I must refer the reader primarily to the second chapter of the third edition of my *Spiele der Tiere,* which appeared in 1930.[17] Some of what I said there, drawing upon the work of Lloyd Morgan, McDougall, Buytendijk, Alverdes, and other biologists, may be stated here as follows: The construction (corresponding to the building-block point of view) according to which the instincts are made up of reflex movements (Spencer, Jacques Loeb, Ernst Mach) is rejected (pp. 30 ff.). True, one may, for instance, in relation to the instinct of nutrition, which consists in a unified behavior of the entire organism, distinguish separate elements (biting, chewing, swallowing, etc.) which may at times appear alone and are then called reflexes. But one would be reversing the picture of development if one tried to interpret this fact in this sense. I said, therefore, that the phenomenon so isolated should not be genetically interpreted as an original building-stone, and that it would be "better to assume that the reaction busying the entire organism is present from the beginning and, as a unit, has become more and more highly differentiated, so that the partial functions in this unity may sometimes also manifest themselves alone" (*loc. cit.,* p. 31).

The separation of reactions into inborn instincts and acquired behavior based upon experience (*Erfahrungshandlungen*) also needs correction, for it has led to regarding many animals as pure automata or "reflex machines." The observations of zoologists are making it clearer and clearer that even the animals most pronouncedly ruled by instinct (*Instinkttiere*) are guided by perceptions and learn from experience. Consider, for instance, the more recent investigations, mentioned before (*loc. cit.*, p. 12), on the lion ant (*Myrmeleon formicarius*), formerly considered the very pattern of a "reflex machine." Therefore, in my opinion, the custom of designating animal reactions as a whole as "instincts" and distinguishing them from acquired reactions is to be regarded as a *denominatio a potiori* which, for practical reasons, cannot quite be avoided, but which is not quite correct. A more accurate definition would have to read as follows: the concrete animal reactions are instinctive insofar as they are the expressions of innate dispositions characteristic of the species. This relativistic definition with its "insofar-as" was already present in the first edition of my book—a fact which has been overlooked by several critics. It receives gratifying confirmation in the excellent exposition which Alverdes has given in his *Tiersoziologie* (1925). If we designate the concrete action of the animal by A, the element in it that is purely instinctive because it is hereditarily determined for each individual of a species and is therefore constant by K, and the variable element of individual evaluation of experience by V, the whole process can be expressed by the formula: $A = f\ (K, V)$. The analysis into K and V is purely a rational distinction. The functional relation between the two factors varies. In the so-called animals of instinct (*Instinkttiere*) $V<K$; in the animals with actual learning or initiative (*Lern- oder Initiativtieren*) $V>K$. This is to say at the same time that in human beings also the instinctive side of reactions is present, and is merely filled out much more extensively by acquired experience. Even in the most abstract thought processes the instinctive element may have great influence. Consider, for example, the selective and directive power of the instinct of pugnacity in discussions apparently dominated solely by logical arguments.

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[17] All the theoretical parts of the book were entirely rewritten. The English translation of 1898 is therefore no longer adequate.

The instincts, in the further exposition of which I have frequently started from McDougall's point of view, must be distinguished from imitative actions. Impulsive (*triebähnliche*) imitation, as it shows itself, for example, when the infant responds to the smile of its parents with its own smile, or rounds its mouth, sticks out its tongue, or shuts its eyes tight when it is shown how to do these things, places before the psychologist many a puzzle which is difficult to solve. I would simply refer the reader to the discussion in the second and sixth chapters of my *Spiele der Tiere* if I were not eager to call attention here also to a difficulty which I could not master then and for which even now I know no sure solution. The situation is this. In some cases one might hope to deduce imitation from a more general tendency to execute every strongly ideated movement. (For instance, a person with a lively idea of whistling feels an impulse actually to whistle. The same thing happens, however, when we hear others whistle, and this is what we call the instinct of imitation.) Such an explanation seems to be impossible, however, for our examples. For the impression which the infant gains of the strange, noiseless movements of the lips and eyes is a purely visual one; on the other hand, unless we assume that he is watching himself in a mirror, he cannot see his own reactions at all, and has at his command only kinaesthetic images of his smiling, of pursing his lips, etc. How, then, is the connection between the visual impression and the kinaesthetic image to be explained? The friend of my youth, the philosopher Julius Schultz, wrote to me after the appearance of the new edition of the book previously mentioned, that he regards this point as one of the most puzzling in the whole field of psychology. For he agrees with me in the view that the attempts of Thorndike and McDougall, in regard to the doubtful reactions, to assume "special innate connections" or "simple rudimentary instincts" in place of the instinct of imitation cannot well be carried out. How should nature develop a special instinct which causes the infant to purse his lips or screw up his eyes (as William Stern saw infants do) when he sees others make the gesture? In regard to the answering smile, we might perhaps speak of such special inborn connections; or we might, as Schultz wrote me, assume that, since the father often smiles at other times, too, in playing with the child, the latter has developed an association between the sight of the smile and a pleasant mood, so that this mood of itself, without real imitation, induces the smile of the child. But such an explanation fails in regard to the contagiousness of extending the tongue, pursing the lips, or screwing up the eyes before the child. Shall we bring in the observation in the mirror after all, or doubt the findings of psychologists in general? Both procedures promise little success. Or are we to think of a telepathic transfer of the motor excitements? I know of no satisfactory solution of the puzzle.

In the studies of play, in agreement with Schultz, I have emphasized the idea that in all play activities the inherited and the acquired are combined. They manifest considerable differences, however, which may lead to a classification. The decidedly instinctive plays, such as hunting, fighting, and courtship games, can be clearly distinguished from the imitative and experimental games, since the former, with their greater independence of the acquired, and their more pronounced utilization of new adaptations, represent reactions which, even in animals, can be recognized as preliminary steps toward what in man leads toward the development of culture. This coincides with the fact that imitating and experimenting reveals its full significance only in the animal species with the highest intellectual endowment. Since I cannot discuss these connections more fully here, I would like only to point out the influence of imitation and experimentation (outside the field of play as well as in) upon human culture.[18] The imitation of the members of one's own species leads beyond what is innate and transmits to the new generation the life habits acquired by preceding generations. Baldwin has called this transfer through imitation "social inheritance," in contrast to true physiological inheritance. The finest example of the former is the way in which children learn the speech of adults. If imitation, sociologically considered, is thus a principle of conservation, because it maintains the adaptations of the parents and ancestors, then the handling of objects (Stout calls it "manipulation") already designated by Rousseau as experimentation, which expresses itself in taking apart and putting together again, among other ways, is a principle of individual progress leading to discoveries and inventions perhaps still unknown to preceding generations. Since the two tendencies, although both are present in all people, are present in different proportions, they shed some light upon the contrasts between political parties. The man whose nature urges him toward radical progress is obviously not free from the influence of imitation, but he would like to pull down the existing national edifice and build up a new structure independent of all tradition. The conservative nature, on the other hand, hangs lovingly upon tradition and conceives of progress more organically, as it were, than does the radical, regarding it as a continued growth of the ancient order toward fuller consummation.

I have already said that I cannot discuss more fully my exposition of the various types of play. Nor will I discuss again now the definition of play,[19] the causes which call it forth, and the life values with which it is associated, since I treated them recently in the new edition of my animal psychology. Let me simply state that, in considering the biological expressions of play, even today I am dominated by the thought already expressed in Plato's *Laws*, that play in youth can be regarded as practice for future life tasks, which is carried on by the growing individual without his having any idea of the ends which are realized by the activity which he undertakes simply for the sake of pleasure. In the third chapter of the book previously mentioned, this unintentional self-education is distinguished from other types of evolution (cf. also *Das Seelenleben des Kindes,* p. 78) and supplemented by an enumeration of the other most important life values of play. One of these life values, namely, the function of "catharsis," emphasized by Carr and Claparède, is important particularly for the understanding of maturity, to which, incidentally, I also devoted a special little study in the *Internationale Monatsschrift* for 1912.

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[18] Cf. *Der Lebenswert des Spiels* and *Die Spiele der Tiere,* 3rd ed., p. 53.

[19] In connection with this, Kohnstamm now rightly utilizes the manifestation of spheres of experience, already mentioned (cf. p. 141 above).

I have attempted to follow the behavior of the instincts and the imitative impulse into the field of aesthetics. I would like to say here, in advance, that I regard it as a question of definition whether psychological research on the creation and enjoyment of agreeable objects should be counted as part of aesthetics or not. On the other hand, I am fully convinced that, whether or not one classifies such studies as aesthetic, their importance and necessity must in any case be conceded. Epistemological aesthetics and metaphysical aesthetics require empirico-psychological determinations if they are not to remain floating in the air. If "psychologism" rests upon limited insight, the disregard of psychological contributions to the knowledge of the beautiful likewise suffers from this widespread disease. Insofar as the influence of instincts is concerned, I would like to refer the reader to the discussions in the books *Die Spiele der Menschen* and *Der ästhetische Genuss,* in which I tried to show how strongly especially in poetry, the need is expressed of satisfying, in the imaginary world of fantasies, the fighting instinct and the very manifold impulses relating to sexuality. Naturally, "art" only begins with the beautiful moulding of material. But one must also know what contents are here primarily involved. This importance of the content underlying artistic form probably shows itself most clearly in the comic and the tragic. Since this discussion is intended for English readers, it may suffice to quote a characteristic section from Samuel Butler's *Hudibras* (cf. *Der ästhetische Genuss,* p. 247):

"There was an ancient sage philosopher
That had read Alexander Ross over,

And swore, the world, as he could prove,
Was made of fighting and of love.
Just so romances are, for what else
Is in them all but love and battles?"

In order to understand artistic production, it seems to me to be necessary to consider not only the principle, artistic in the narrower sense, of the aesthetically satisfying form, but also the motives of imitation and self-expression connected with the instinctive life. From the first beginnings these three factors make themselves felt in the arts with varying emphasis, imitation in the reproduction, however free, of the present reality, and self-expression as the deep-rooted urge to express the inner life of the creator in the artistic form and the effort to mould material in such a way as to attain an arrangement of forms adapted to the content thus given. From the third principle there are roads leading into metaphysics (cf. particularly the essay "Enkapsis" in the *Zsch. f. Psychol.,* 1926). Since all reality, inorganic, organic, and mental, manifests a tendency to the formation of structures (in the indicated sense of a hierarchy of forms [*Gestaltungen*]), it seems reasonable to interpret the activity of the artist as a manifestation of the same powers which in the universe press from Chaos toward Cosmos. In my essay on Flaubert's novelette *Un Coeur Simple*—an essay which first appeared in Volume 18 of the *Zeitschrift für Aesthetik* (1924) and was incorporated in the same year into my *Beiträge zur Aesthetik*—I tried to demonstrate the expression of all three principles in the analysis of a single work of art.

The activity designated as "internal imitation" (Jouffroy, too, uses this expression) was already made the focal point of the whole investigation in the aesthetic work of my youth (*Einleitung in die Aesthetik,* 1892). In speaking of "play of internal imitation," I attempted, as was already mentioned, to make a double synthesis: first, the union of Lotze's and Robert Vischer's description of inner empathy (*innerliche Miterleben*) with Schiller's theory of play, and, secondly, the biological relation of the concept thus supplemented to the instinctive life of man. It was not until later that I recognized that (quite apart from the attitude of critical evaluation which leads to the aesthetic judgment) there is also another type of enjoyment which is characterized rather by quiet looking than by that active participation which I have even called "the artistic re-creation"

(*Nacherzeugung*) of perceived forms.[20] This does not, however, keep empathy from remaining one of the principal problems in psychological aesthetics. It was a satisfaction to me to be able to ascertain, in running through the history of aesthetics, how many leading aestheticists have known from their own experience and emphasized the physiologically conditioned behavior which is now designated as inner empathy or inner imitation. Particularly the English aestheticists of the eighteenth century, Hutcheson, Hogarth, Burke, and Home, gave characteristic examples of this. Further detail will be found in the essay "Das innere Miterleben in der älteren Aesthetik" (*Ann. d. Phil.,* 1922).

In the essay last named (but also in the *Spiele der Tiere,* pp. 195 ff.) I returned to a hypothesis (already defended in *Der ästhetische Genuss*) which classifies kinaesthetic inner empathy in relation to larger biological connections.[21] It takes as its point of departure the fact that, in addition to the imitation which is connected immediately with the model, there is a delayed imitation which only appears later. This delayed imitation is extraordinarily important because, as William Stern has also brought out, it forms the child's principal method of learning; but it presents the psychologist with a puzzle. How can it be explained that the child suddenly copies a movement which perhaps it last saw several days before and did not then externally imitate? How, to give a particularly remarkable case in the acoustico-motor field, could the approximately three-year-old son of Stumpf, the psychologist of sound, who up to that point had clung stubbornly to a language of his own invention, suddenly surprise his parents with an almost faultless German? Mere direction of attention does not suffice to explain this, even apart from the fact that delayed imitation may extend to movements and postures by which one becomes "infected" without any special arousal of attention. Here the hypothesis seems almost unavoidable that the later external imitation was preceded by a delicately innervating inner imitation which was immediately connected with the model and formed a sort of rehearsal for the subsequent external imitation. If this is correct, internal imitation is a type of behavior which is at first outside of the realm of aesthetics but which appears in all children and is an eminent necessity in life.

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[20] Cf. the essay "Das ästhetische Miterleben und die Empfindungen aus dem Körperinnern." *Zsch. f. Aesthetik u. Kunstwiss,* 1909, vol. 4).

[21] The hypothesis presupposes real innervations of an imitative character. In the essay on empathy in the older aesthetics I have touched upon the possibility that true (even if only weakly indicated) innervations may sometimes be replaced by their "eidetic" reproduction. Whether this is true in my own case I have begun to doubt. In any case, the genetic psychological assumption made here applies only to real movement processes.

The development of this general, not yet aesthetic, imitation into the type of grosser and more delicate aesthetic empathy could then be pictured as follows:[22] The child finds out that the kinaesthetic imitation of seen or heard forms, by giving rise to empathy with what is perceived, possesses a not inconsiderable pleasure value, as, for example, when we accompany our listening to a poem by an emotionally tinged recital of it to ourselves, or when, in watching a workman executing rhythmic movements, we make rudimentary movements along with him.[23] Internal imitation, through the pleasure value inherent in it, thus becomes an activity akin to play. When the instinct of imitation, the height of whose power is reached in the period of childhood, recedes, the habit of inner imitation is gradually lost by many people, or at least makes its influence felt but little; then the more contemplative type of behavior arises. Where the instinct remains in action longer, however, empathy may attain a form of development in which the interest in mere material recedes in favor of the intellectualizing pursuit of beautiful form, that is, it may change into a type of refined aesthetic enjoyment. It is precisely with this, rather than with the grosser manifestations of concrete empathy, that I have always been primarily concerned. The introspections upon which the descriptions of inner imitation in Lotze and Robert Vischer, in Vernon Lee and Anstruther-Thomson and in Wölfflin and Hildebrand are based come from finely formed natures of strong aesthetic receptivity and relate not to what is material but to the most intimate charms of form. Thus an important type of aesthetic behavior appears to be related to the fact that the play of inner imitation is retained long enough in some people to be able to master beauty of form.

I shall just point out three other things connected with the emotional life. The first and second, which are discussed in my *Seelenleben des Kindes,* will merely be mentioned here: the biological significance of pleasantness and unpleasantness and the emotions relating to fear. Both discussions exceed the scope of child psychology. I regret that I cannot go into them more fully in the present exposition. The third problem concerns the experience of the stirrings of conscience. This is treated in the already-mentioned essay "Die Stimme des Gewissens: zwei psychologische Stichproben" (*Zsch. f. Psychol.*, 1928, vol. 108). In my first experiment (winter of 1912-1913) I asked 79 auditors and in my second (summer of 1928) 84 students to consider the meaning of the words "the voice of conscience" or "a qualm of conscience" and then to answer two questions: (1) Did you think in this connection of good actions or bad ones, or both? (2) Did you think of the judging voice which pertains to what has already happened or of the warning voice directed toward the future or of both? From the answers to the first question it appeared that H. G. Stoker (in *Das Gewissen,* 1925) was right when he designated the "bad conscience" as the most "intensive" phenomenon; nevertheless, to my surprise, in the second experiment ten people thought only of the "good" conscience. I was still more surprised at the relatively large number of people who assured me that they imagined only the conscience directed toward the future (*conscientia antecedens*). My results were then checked by experiments, still unpublished, by Willy Moog (*Versuche mit Zuhörern*) and Max Dessoir (*Fragen im Rundfunkvortrag*). On the second question Dessoir received 325 utilizable answers, of which 169 went to the count of the conscience that judges the past and 82 to that of the conscience which warns for the future. Since I myself, if I did not know the literature, would probably think only of the bad conscience following the deed (*conscientia consequens*), typological differences seem here to make themselves felt which invite the conclusion that we must be cautious about isolated *Wesens-schau* (introspection?) in such cases.

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[22] *Das innere Mitleben in der älteren Aesthetik,* p. 403.

[23] I can only indicate here how important internal imitation is for the understanding of strange modes of expression. "As children," says Yriö Hirn in his *Origins of Art* (p. 79), connecting with my work, "we all imitated before we comprehended, and we have learned to comprehend by imitating."

This exposition ought really to end with a few statements regarding my metaphysical views on the soul (*Seele*). But since I shall presently discuss these in another publication, in which I am to give an account, not of my psychology but of my philosophy (*Weltanschauung*), I shall limit myself here to sketching a thought which I briefly presented first in 1917 in the *Untersuchungen über den Aufbau der Systeme* (book edition of 1924, pp. 297 ff.), and most recently in the pamphlet *Methodik und Metaphysik* (1928). The ordinary psychophysical parallelism, as E. Becher in particular has made clear, is subject to the danger of splitting up the psychic into separate little "soul splinters" (*Seelensplitterchen*), a condition which does not correspond to the unified nature of mental life. This danger is avoided in the monadological monism represented by Adickes (and similarly by Sapper), according to which the soul is the inner side of a single highly developed primary atom (*Uratom*). The same danger is avoided, however, when the psychic is brought into relation not with the inner nature of the discrete world elements, but with the continuum which is intuitively (*anschaulich*) given us as the field of space. That which we designate as empty space (the "kenon" of Democritus) would then be, when regarded from within, a spiritual power guiding the world elements, a power which, in hierarchical order,[24] as a conscious soul governs certain processes in the cerebral cortex, as "entelechie of the body" governs the whole organism, and as All-Soul (*Allseele*) governs the Cosmos. (Intuitive space was characterized by Kant, too, in his pre-critical stage, as *"Omnipraesentia phaenomenon."*) The hypothesis thus indicated must be completed, however, as I have already pointed out in the pamphlet mentioned (p. 33, note 2), by the connection with time. Leibnitz has designated space as *"ordre de coexistence."* According to the hypothesis here sketched, the soul is not only the power which arranges the present (*das "Zugleich"*) but also the *"ordre d'evolution,"* ruling past and future, which is given us intuitively as time. Metaphysically considered, space and time would here have a unified principle behind them, while Minkowski has combined the two concepts from a mathematico-physical point of view.

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[24] The essay "Enkapsis" (*Zsch. f. Psychol.*, 1926), and in part also the uncompleted essay on "Die Lehre vom unfassenden Seelensein" (*Zsch. f. Psychol.*, 1918), is connected with the interest in this hierarchical order.

GERARDUS HEYMANS*†

If I were asked to express as briefly as possible, just what differentiates my philosophical researches from those of most of my contemporaries, I should reply: *The fact that these researches consistently employ empirical methods,* and *yet arrive invariably at anti-empiristic conclusions.* This may appear as a paradox, perhaps even as a contradiction—but only because people are always misled by the similarity of the words "empirical method" and "empiricism" to ignore the difference between the correlative concepts. The difference lies in the very fact that the empirical method is a *method,* a scientific procedure, which consists in the universal attempt to base every research upon the most exact and exhaustive observation of the actual facts of experience; whereas empiricism is an *epistemological doctrine,* to the effect that our knowledge can never exceed the bounds set for it by the facts of experience. These are obviously two very different matters. In employing the empirical method we need not by any means assume that the facts with which we commence are never going to point to anything beyond themselves; as indeed natural science, the paragon of all empirical research, has always held the faith that by its explanatory hypotheses it could attain to knowledge beyond the immediately given. On the other hand, one who accepts empiricism may very well have reached that attitude through general considerations, i.e., by conceptual rather than by empirical means. It might be said, in general, that the method does not in any way prejudice reflections concerning its results; one should simply employ it conscientiously, and wait upon the resultant conclusions.

That the objects wherewith the several philosophical sciences are concerned are in some respects amenable to empirical treatment, no one is likely to deny. The facts of theoretical thinking, and of ethical and aesthetic evaluation, can certainly be described, ordered, reduced to general laws, and complemented by explanatory hypotheses, just as well as natural phenomena; one might also ask how

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we must conceive the world in order to account for the totality of our available data, in the manner in which natural science and psychology try to account for the phenomena of nature and of consciousness, respectively. Of this, as I have already remarked, there can hardly be any doubt; but, on the other hand, it is frequently said that the aims of those "normative" sciences which deal with the first-named topics, i.e., epistemology, ethics, and aesthetics, can never be attained in this direction. For, besides correct thinking, there is also that which is incorrect, good taste is everywhere rivalled by bad taste, the ethical views of different races, times, or individuals are widely divergent: under those conditions, empirical research might indeed bring to light an infinite variety of logical, ethical, and aesthetic standards, might even try to explain the actual validity of these by considerations of hereditary and environmental influences, but could never, save by majority vote, reach a conclusion as to the criteria by which judgment and valuation *should* be guided. But just that is supposed to be the function of the normative sciences, and therefore these sciences cannot hope for advancement by the empirical method. This line of argument entails, in my opinion, at least three fallacies. In the first place, it ignores the fact that individual differences in the actual phenomena of judgment and valuation do not exclude the generality of the criteria involved, any more than the contrast between the fall of a stone and the soaring of a gas balloon excludes the generality of the law of gravitation. For our criteria, as well as this law, do not in themselves determine phenomena, but only in conjunction with special conditions, the relevant accompanying phenomena; just as the law of gravitation under different conditions can produce contrary motions, so the same criterion under different conditions might evoke contrary judgments and views. Indeed, we often find this to be the case: find that theoretical differences rest entirely on the dissimilarity of the data available or taken into consideration, ethical disagreements to be based on different interpretations of the acts in question, so that both disappear when the facts of the judgment or valuation are once agreed upon. That this would, in general, be the case, we might infer from the fact that differences arise almost only with regard to complicated cases, where a multiplicity of data or of possible interpretations enter in, whereas agreement increases with increasing simplicity of the cases in hand. In the second place, it is often taken for granted that the empirical method can aim only at "genetic" explanations, i.e., at a reduction of our theoretical, ethical, or aesthetic insights to *something alien*, as, for instance, to accidental associations or hedonistic feelings. But that is an error: empirical research, in spiritual life as well as in nature, can reveal eternal laws just as much as historically grounded, causally determined regularities; in fact, its constant concern should be to correlate the latter with the former. In regard to the question we are discussing, that means: it is just as conceivable that our words "true," "good," and "beautiful" denote conceptions which are rooted in our inmost beings, and, as such, have a general and invariable content, as that they refer only to accidental products of evolution, which at bottom are compounded of entirely different elements from what our introspections lead us to believe. Thus, in respect to explanatory principles, too, the empirical method is entirely non-committal on the subject of what we may expect to find. In the third place, finally, there are certainly many persons who, if perchance empirical science could prove the generality and fundamentality of such norms, would still inquire whether the way in which judgments and valuations *are* performed (i.e., according to these norms), is the way they *should* be performed. But this question, too, is not rational. For if that which we *mean* by the words "true," "good," and "beautiful" is clearly revealed to our consciousness along these lines, there would be as little sense in looking for a higher sanction to instate our opinion, as in seeking a higher sanction for the concepts we mean by the words "red," "sweet," or "triangular." And if, taking all unambiguous data into consideration and excluding all extraneous conditions, we apply those concepts to a given case, the result of this application in itself entails the unquestionable insight that anyone who uses these words in the same sense as we must come to the same conclusions. If, however, he meant something else by those words, why, then he would be talking about something else, and his conclusions could no more contradict ours than corroborate them. These, then, are the three errors which are inherent in almost every opposition to "psychologism" in the normative sciences; once we are emancipated from them, it no longer seems quite hopeless to test the power of the empirical method in these fields. What seems to me the outcome of this test shall now be briefly stated.

In the first place, concerning the theory of knowledge,[1] I find myself in direct opposition to most of the philosophers and philosophical schools of my day, in that *I resent on principle any attempt to "remodel the conception of knowledge," and always mean by "truth" just what from antiquity has always been meant by that word, namely, the correspondence of our ideas with that reality to which we refer them in the act of judgment.* That, in itself, is a purely terminological matter, but even as such is not unimportant. Anyone who thinks that the results of his thinking are not directed at truth in this sense, but merely at a simplest description of the given, or practically useful ideas, or thought-connections accompanied by a sense of obligation, should avoid misunderstandings by not denoting such results by the word "truth" at all, but with some other of the many available and suitable words. And this especially because we *cannot dispense* with the traditional conception of truth, and therefore always need a name for it. Whoever makes an assertion about anything directly given to his experience (his own impressions, thoughts, moods) can and does certainly always mean to imply that this really was the case; and we cannot fail to breed confusion if, by the truth of other assertions, we mean something quite different.

But even apart from this question of names, the application of the empirical methods to epistemology, as outlined above, makes it immediately evident that we must stick to the old conception of truth. For this method proposes to start with the facts of thinking, as these are clearly and adequately exhibited in our existing science, and it goes without saying that it must take these facts *just as they are exhibited,* i.e., as assertions regarding a reality independent of our ideas. The convincingness, true or apparent, of these assertions, is what epistemology must first of all account for, i.e., trace it to its source, and, furthermore, examine in regard to its validity; but this explanatory account may (though it *need* not) contain the proof of that validity. For our convictions are certainly based at least in part upon sufficient reasons; and, wherever their origin can be accounted for by such sufficient reasons, they are explained as well as justified. At all events, wherever self-evident conviction seems to obtain without sufficient reason, epistemology meets with a problem for which it must seek a solution in one direction or another.

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[1] Analytisch, synthetisch. *Vjsch. f. wiss. Phil.*, 1886, **10**. Zur Raumfrage. *Vjsch. f. wiss. Phil.*, 1887, **12**. Einige Bemerkungen über die sogenannte empiristische Periode Kants. *Arch. f. d. Gesch. d. Phil.*, 1888, **2**. Erkenntnistheorie und Psychologie. *Phil. Mon.*, 1888, **25**. Noch einmal: Analytisch, synthetisch. *Zsch. f. Phil. u. phil. Kr.*, 1889, **96**. Schets eener kritische geschiedenis von het causaliteitsbegrip in de nieuwere wijsbegeerte, Leiden, 1890. Die Gesetze und Elemente des wissenschaftlichen Denkens. Ein Lehrbuch der Erkenntnistheorie in Grundzügen. Leiden u. Leipzig, 1890. (3. Aufl., Leipzig, 1915.) Über Erklärungshypothesen und Erklären überhaupt. *Ann. d. Naturphil.*, 1902, **1**. De geschiedenis als wetenschap. *Bijdr. Kon. Acad. v. Wet.*, 1906. De psychologische methode in de logica. *Tijdschr. v. Wijsb.*, 1908, **2**. De empiristische ruimtetheorie. *Tijdschr. v. Wijsb.*, 1912. Natuurwetenschap en philosophie. *Tijdschr. v. Wijsb.*, 1916, **10**. Prof. v. d. Walls en de theorie van Hamilton. *Tijdschr. v. Wijsb.*, 1918, **12**. De wetenschap en de andere cultuurwaarden. *Onze Eeuw*, 1919, **19**. Leekenvragen ten opzichte van de relativiteitstheorie. *De Gids*, 1921, **85**.

As a guiding thread toward the settlement of the question, what sort of scientific judgments such problems entail, the old Kantian division of judgments into analytic and synthetic ones, and of the latter into synthetic judgments *a priori* and *a posteriori,* still seems to me well-nigh indispensable. This classification has sometimes been criticized as arbitrary and unessential, but this criticism overlooks the fact that ideas, not words, compose the judgment. It is true that a *proposition* may convey either a synthetic or an analytic judgment, depending on how we define the words which occur in it; but what the speaker *means* is always the demarcation and accentuation of a characteristic already contained in the definition, or the actual synthesis of several characteristics or groups of characteristics which are not initially contained in one another. In the first case the judgment is analytic, and as it is concerned fundamentally only with the use of language, requires no further elucidation. In the second case, it is synthetic, but needs just as little explanation or justification, if it is synthetic *a posteriori,* as an analytic judgment, since it merely asserts what is given in experience. If, however, there should be synthetic judgments *a priori,* whose validity could no more be found in the constituent concepts than in experience, then the question would, of course, be in order, what does validate such judgments, and whether the guarantee be acceptable. *And thus it is synthetic judgments a priori that are the real object of interest to epistemology.*

Now it does appear, superficially at least, as though such synthetic judgments *a priori* did occur in all the sciences, and even occupied a central position. In the mere act of applying logical laws to reality, i.e., in assuming that a conclusion from correct premises must itself be correct, we appear to be postulating some harmony between the laws of the world and of thought, which, on the one hand, obviously transcends the abstract concept of the latter, and, on the other, overflows the bounds of accessible experience by reason of its absolute generality. The case of *arithmetic* is similar, in that according to Kant even the simplest addition formula equates two numerical values, without any possibility of one being given in the idea of the other so that it could be analytically derived therefrom—likewise in *geometry,* as the Euclidean axioms present characteristics which could logically be separated as inseparably connected. As here space, so in *kinematics,* time is the subject of synthetic judgments *a priori,* as we use for instance notions of its infinity or its irreversibility; in mechanics, too, the fundamental principles of the science, as, for instance, the laws of inertia or of the parallelogram of forces, have often been regarded as self-evident without the aid of experience. And, finally, in the empirical natural sciences we always assume at least the law of causality to be acceptable without proof, though the mere concept of change as such gives us no reason at all to suppose that the change must have a cause. In all these cases epistemology should determine, first of all, whether synthetic judgments *a priori* are really involved, and if so, should discover their exact content, and finally seek an explanation, if possible, a vindication, of their presence.

Now it seems to me that so far epistemology has not treated the first two of these tasks, i.e., the careful and exhaustive inquiry into the actual presuppositions of science, with adequate, painstaking precision. Thus, for instance, the wealth of material concerning the foundations of geometry, which the researches of Riemann and Helmholtz have revealed, have either been neglected, or have been used only to form premature conclusions as to the *a posteriori* character of geometrical knowledge; and, similarly, in treating of the principle of causality, attention has been given almost exclusively to the formal characteristic of temporal succession, not to the important presuppositions regarding the material relations of cause and effect, which prove to be particularly relevant in the construction and testing of explanatory hypotheses. In general, epistemologists have underestimated the importance of the study of actual thinking for the establishment of norms of thought, and have thus unnecessarily weakened their chances of transcending positivism and skepticism. He who can see only such ways as certainly do *not* lead him to the truth, should not fail to explore the ways of others who profess to have found truth in these directions.

In regard to the ways in which people have attempted to solve the problem of synthetic *a priori* knowledge, two main tendencies seem to prevail among the philosophers and philosophically minded scientists of the present day, but, on account of the above-mentioned neglect of the facts of actual thinking, neither of these proves adequate either to the explanation or the justification of such knowledge. One of these is the *empiricist* tendency, which seeks everywhere to reduce synthetic *a priori* judgments to the synthetic *a posteriori* type; i.e. (following Mill's example), to treat all logical, arithmetical, geometric, and mechanical laws as inductive generalizations of experience, and claim the possibility of basing this inductive generalization itself on separate experiences, or else explaining it in terms of association of ideas. That a satisfactory explanation of scientific certainty cannot be found in this direction is clearly evinced by the apodeixis, the absolute generality and precision of logical and mathematical judgments as compared with the purely contingent and approximate character of even the best-authenticated empirical judgments, as also by the fact that the intensity of ideas, which may be augmented by associative ties, is not (as Hume supposed) paralleled by the intensity of conviction. And neither can we found a *justification* of universal knowledge—even disregarding its super-empirical certainty—on individual facts, since there would be no contradiction involved in the supposition that in any given number of cases, $A_1 \ldots . A_n$, a certain property B were present, but that in a further case, A_{n+1}, it were lacking. But if, in completion of the proof, one were to assume an equally inductively given uniformity of nature, that would, of course, be argument in a circle. The second of the above-named tendencies might be called the *logistic* tendency; because it arranges the definitions of scientific concepts in such a way that the universal axioms are implied therein with logical necessity. Thus, for instance, Grassmann and Hankel define the arithmetical sum of a and b as that member of the number-series for which the proposition

$$a+(b+1) = (a+b)+1$$

holds, and may, consequently, rest assured that, whatever they are dealing with a sum of this sort, they may apply this formula. In this way arithmetic seems to be established at one swoop as an analytic science, whose apodeictic, absolutely general, and exact nature would be both explained and justified. In the same way one might proceed in the other sciences, by including in the definition of the straight line its determination by two points, in that of time its irreversibility, in the notion of change its causal determination, in order to derive them logically afterwards. But whoever remembers what we said above about the Kantian distinction between analytic and synthetic judgments must see at once that in this fashion not a single synthetic judgment can be got rid of. For a synthetic judgment is one that asserts that, wherever a certain property is found, a certain other property is also present, and is independent of the former, as, for instance, that which we mean by a "sum" always entails the validity of the formula given above; now, if the latter is included in the former, then our proposition merely asserts that the formula holds where in fact it does hold, but not that it always holds where we have a sum in the old sense. What we have explained is nothing more than a product of fancy designed to fit just this explanation; but the certainty of arithmetic has in no way been elucidated. And the same would be true wherever one sought to apply a similar sleight-of-hand.

But if we remain in the given sciences and make an exact inventory of the unproved presuppositions, which they employ in addition to their definitions and empirical data, we can prove in every case, or at least render very probable, the fact that the certainty of these presuppositions, though apparently synthetic *a priori,* can always be based either on freely constructed concepts, or on given or hypothetically tenable facts of thinking or perceiving, and must therefore be accepted as either analytic or as synthetically *a posteriori* (which, however, do not deal with the given concept of experience, but purely with its subjective factors). Concerning the way in which this can be demonstrated, I shall now relate the essentials.

First, we shall observe the laws of logic and inquire how we may know *a priori* that they are universally valid—i.e., that if the premises of a syllogism are taken from experience, the conclusion must also be contained in experience. This question would be hard to answer if the novelty which is contained in the conclusion dealt with the existence of any other phenomena than those described by the premises; for it would be baffling indeed to ask how we could know that to every formal connection between premises and conclusion in our thinking there must correspond some connection between the corresponding realities. But in fact, as one can see in every scholastic example, the case is different. The conclusion never deals with other, but always with precisely the same, phenomena as the premises; *the laws of logic only apparently connect different phenomena, in truth they connect merely different views of the same phenomena; and that it is always possible to take such different views lies ultimately not with the content of experience, but with the anatomy of our thinking.* It rests upon the fact, to put it concisely, that our thinking can employ the function of *negation;* that therefore it can render one identical fact either in the form: "This is *A*," or in the other form: "This is not non-*A*"; as indeed the two fundamental laws of thought, the principle of contradiction and the law of excluded middle, simply assert that wherever *A* holds, non-*A* fails, and wherever non-*A* fails, *A* must hold. Thus the logical laws are not, as has often been asserted, laws of thought as well as of nature, but are exclusively laws of thought; there objects are not natural phenomena, but merely such judgments as the human mind forms *apropos* of the phenomena of nature, and even these judgments only insofar as they depend on the organization of thought. Wherefore one can as little say that the laws of logic are corroborated by experience, as that they are contradicted by it; for experience supplies only phenomena, not perspectives of them; but the laws of logic do not tell us how phenomena are related—only how various perspectives of phenomena are related. In spite of this (or, rather, just because of it), the application of logical laws to any objects of experience can never give rise to error. Just as, when a given set of objects is viewed first through a red and then through a blue glass, one can predict with apodeictic certainty what color in the one case will correspond to any given color in the other, one can also tell that if certain judgments are true concerning some content of experience, then certain others must also be true of it, and are logically obtainable from the former. And the latter as well as the former certainty is explained and justified by the fact that its origin is in the condition of the apparatus, respectively, of perception and of thought, and quite independent of any given object.

The situation in arithmetic is similar and yet not quite the same as in logic. The similarity lies in the fact that *in arithmetical propositions, too, it is not* (as Mill believed) *various phenomena,* *but various perspectives of identical phenomena that are represented as necessarily connected;* but the difference lies in this, *that the possibility of these different views is not founded here, as it is there, upon given and inevitable arrangements of the mind, but rather upon the existence of an arbitrary, but generally accepted and premised instrument of counting, namely, the number-series.* This number-series (as also its forerunners, the fingers of the notched stick) is nothing more than a parameter whereby we measure and compare different groups of phenomena with respect to their numerosity, just as by means of the yard-stick or the scales we measure them with respect to their length and weight. The assertion: "Here are ten books," means simply: "The number of these books resembles that of the digit-names from one to ten," or (according to Frege): "These books may be correlated with those sounds one by one without remainder:" exactly as the expression: "This room is ten yards long" simply asserts that the length of the room is equal to ten applications of the yard-stick and may be brought into exact correspondence to these. This fact explains and justifies in a very simple manner the fact that, and the degree in which, we ascribe necessary and exact validity to the propositions of pure and applied arithmetic. For wherever we apply an arbitrarily selected standard to any phenomenon, we can judge of the properties of the standard in themselves analytically and *a priori,* of the properties of objects which we measure with it, only synthetically and *a posteriori,* of the relations among different ways of describing the object by our standards, however, again analytically and *a priori:* that, for instance, 1 meter = 10 decameters we know with perfect exactness and absolute certainty; that a given object is one meter long, we know only approximately; but that all objects which are one meter long are ten decameters long, we know again with unfailing precision. The same is true in arithmetic. Pure arithmetic deals exclusively with the standards, i.e., with the number-series in and for itself; if it tells us that $7+5=12$, that means merely that the digit-names one . . . seven, one . . . five, have the same numerosity as one . . . twelve, and this assertion is perfectly valid because it follows analytically from the arrangement of the postulated, therefore perfectly familiar and invariable, number-series. But if Kant believed that he was here dealing with a synthetic judgment, this was simply because he forgot *to include in his concept of the individual the universally* *postulated number-series.* In contrast to this pure arithmetic, *applied arithmetic* turns toward the world of phenomena, and this it does in a two-fold fashion. In the first place, by *counting them,* i.e., measuring their numerosity by the standard of the number-series; the result of this, however ("these are seven books"), is synthetic–*a-posteriori,* only factually valid judgments. In the second place, by applying not only arithmetic *concepts,* but also propositions, to experience, for instance, in concluding that, since 7+5=12, likewise 7+5 (or whatever the number) of books must be equal to 12 books. Here we are fully aware again of the apodeixis of the judgment, but here we are dealing again just with the several ways of rendering the same phenomenon in terms of our standard. For it is the same set of books that we count as 7+5 or as 12, i.e., that we put into one-one relation with the digit-names 1 . . . 7, 1 . . . 5, and with the digit-names 1 . . . 12; but the coexistence of these two possibilities follows analytically from the corresponding proposition of pure arithmetic, and has the same validity. This seems to me to solve essentially the problem of arithmetical certainty; only the expansion of the number-series by the introduction of negative, fractional, irrational and imaginary numbers requires a further brief comment. Here, once one has understood the short-coming of the logistic explanation, one can scarcely avoid the approach via geometry; but to this round-about approach one can take exception only on aesthetic, not on logical, ground. For, if an equation is valid in the purely arithmetical sense, it is likewise valid for any arbitrary set of objects, consequently also for abscissas; but if it is valid for the latter, then everything must be valid that may be deduced by geometric proofs; and insofar as this is given, in its turn, in the form of equations which assert the equivalence of two numerical values, it may lay claim to purely arithmetical validity. Thus so far, in default of any closer examination of geometrical proofs, arithmetic may be regarded as an analytic science.

If, now, we turn our attention to *geometry,* we find a state of affairs which has many points of contact with the conditions we determined for logic and arithmetic. There is, indeed, an important difference, due to the fact that here we are not dealing with *verae causae* such as a given mechanism of thought or an arbitrarily chosen parameter, which we apply to experience, but that here we require a hypothetical assumption regarding the origin of the psychological origin of the idea of space. Geometry is the science of space; in order that we may realize how it is possible to obtain judgments concerning space which may claim exact and necessary validity, we must know first of all just what we mean by this space, and whence we derive our conception of it. To this question, however, direct self-observation can yield us but a very uncertain answer; therefore, the answer must be supplemented with hypotheses, and I have felt compelled (especially on psychological grounds) to assent to the doctrine of Riehl, according to which *our conceptions of space have fundamentally no other basis than the experience of a triadic qualitative determination of our sensations of motion, which may be produced voluntarily in any arbitrary degree (limited only by external inhibitions).* Whether these motor sensations are of central or peripheral origin, and whether in the latter case they are produced by stimulation of the muscles, the semicircular canals, or other organs, is irrelevant here; all that matters is that we have different sensations according to whether we move our limbs forward or backward, to left or to right, or, finally, upward or downward. If we assume the standpoint of this hypothesis, we shall see the facts of geometric thinking in a most surprising light. In the first place, it becomes evident that (just as we collect the manifold of tones into a two-dimensional order, ranging them according to pitch and volume) we may also collect the manifold of those motor sensations in a three-dimensional system, which is just what we call *space,* and in which every difference in the composition of a complex of motor sensations is represented as a difference of *direction,* every difference of degree as a difference of *distance.* But now we can determine by calculation that the Euclidean axioms (completed and more precisely rendered by the researches of Helmholtz and Riemann) in a general way (with exception of a few problems still in need of clarification) may be deduced as necessary consequences from the conditions assumed in this hypothesis. In fact, it can be proved that *wherever n independent variables can be combined perfectly freely, i.e., in any degree and under any conditions, the resulting relations will be analogous to those which Euclid has postulated for geometry.* Thus in the two-dimensional manifold of tones, for instance, we have the following analogue to the axiom of the straight line: If, from a tone which is determinate in respect to pitch and volume, two series of tones are produced in ever-increasing height and strength, so that the relationship between the increase of the number of vibrations per second and the increase in intensity of vibrations is different for the two cases but constant within each one, then no tone in the first series will be equal in strength and pitch to any tone in the second series. Or for the *n*-adic manifold of a compound of *n* different substances, we might have the following analogy from the same axiom: If, to a certain quantitatively determined mixture of *n* substances, we add gradually part of another mixture, wherein the same substances are contained in a definite proportion; and if, upon another occasion, additions are made to the first mixture from a third one wherein the same elements are contained in yet another proportion; then the results of the first process will not at any stage be equal to the results of the second process in any stage. All propositions of this sort have apodeictic and precise certainty; but whenever it has been supposed that, with especial reference to space, Euclidean geometry might be replaced by some other geometry (spherical, pseudo-spherical, etc.), which would be equally thinkable, *this has always involved the presupposition of limited production of motor sensations, a curtailment or guidance of such sensations through external objects* (as when the hand is allowed to move only over a curved surface). But geometry has always, from antiquity, claimed to be nothing but the science of space, and has relegated external objects to physics. It is, of course, the geometrist's own business if he wants to give up this standpoint, and, instead of investigating the parameter, concern himself with the object of his measurements, and thus identify his science with physics; epistemology is concerned only with the fact that, as long as he dealt with pure space as such, he was perfectly justified to base his science upon the *a priori* necessity of the Euclidean axioms.

About *Kinematics* we have at present little to say. It employs, besides the concepts of arithmetic and geometry, only that of *time,* and in regard to this has certain presuppositions which claim the same sort of apodeixis and certainty as the axioms of the two other sciences. But as long as the psychological origin of the concept of time is still shrouded in mystery, the problem of how to demonstrate the self-evidence of the presuppositions can be met only with general tentative guesses. The guesses rest upon the strict analogy which holds between both the form and the content of our knowledge of space and of time, respectively. The only difference lies in the fact that we regard space as three-dimensional, and time as only one-dimensional; for the rest, however, the properties of infinity, homogeneity, continuity, and constancy of direction belong to the one as to the other, wherefore we can make the same statements about time as about the straight line. And, as such statements have the same claim to necessity and exactness in the one case as in the other, we may fairly suspect that they are based upon a subjective standard, which we do not derive from the phenomena in question, but which we apply to them. But these uncertain suggestions can be given a definite substantiation only as we succeed in determining the elements of the time-sense, as we have determined those of the spatial sense to be motor sensations.

Finally, we come to the *empirical natural sciences* with their fundamental assumption: *Causality.* In regard to this assumption, I have not been able to get beyond the problem of what we mean by a causal relationship; the other question, on what basis we are justified in assuming that in every case of change such a relation must be involved, I have had to shelve for the present. But with regard to the first question, it has—as I mentioned above—been wrongly supposed that those facts of thinking which play a part in the formulation of *empirical laws,* and which merely imply that the causal relation may be conceived as a relation of sequence, were sufficient for its definition. For, besides these facts, we must also recognize others, which appear with particular clarity in the invention of *explanatory hypotheses;* these show us, however, that in the sciences we are never satisfied with mere sequences, but are always anxious to complement or interpret these in such a way that they shall conform, or at least approximate as nearly as possible, to certain presuppositions which our thinking contains in regard to the causal relation. These presuppositions are the spatial contiguity of cause and effect, the equivalence of the two, and the derivation of the effect from the *total* cause (wherefore the latter is temporarily supplemented by the idea of "natural force"). And now it appears that all these facts of causal thinking, as also those of inductive thinking generally, can be subordinated, without exception, to Sir W. Hamilton's hypothesis, that *by the causal relation we mean, fundamentally, some relation of identity of an earlier with a later event.* That this must necessarily be the case might have been inferred from the simple consideration that for every *alteration* we demand a causal explanation; for if every alteration requires such an explanation, this is as much as to say that ultimately such an explanation can be given only by eliminating the alteration, referring it to something enduring and unchangeable. And this is quite in agreement with the fact that from Thales to the present day all philosophers and scientists have striven to find an unchanging substance underlying the mutable world of appearances. Not only upon scientific proceedings in general, but also on the peculiar nature of mechanics in particular (which lies half-way between mathematics and science), the Hamiltonian hypothesis casts a welcome ray of light. To regard mechanics as simply the science of motions and forces is to misconstrue its character; for motions and forces are the subject-matter also of gravitation theory and magnetism, which have always been assigned to physics, not mechanics. The latter, however, *has always been concerned with determining the general conditions which any motion whatever must realize in order to be subsumed under the Hamiltonian principle.* The principle of inertia, the principle of the parallelogram of forces, and that which asserts the equality of action and reaction, assert merely, respectively, that an uninfluenced motion continues without alteration, that the conditions which cause a change in a state of motion must account completely for this change, and that the existing quantity of motion can be neither increased nor diminished. Of course, this does presuppose that no mere change of place, but only a genuine change in the state of motion as such, is to be regarded as a real alteration in need of explanation, and of course the grounds for this presupposition lie in experience; but the fact that these conclusions from experimental premises are convincing to a degree approaching that of the *a priori* sciences, and far beyond that of other empirical beliefs, can be satisfactorily explained only through the fact that they fit the Hamiltonian principle so perfectly. If now, however, we regard the self-evidence of the Hamiltonian principle as the immediate justification of the proceedings of natural science, the problem contained in the latter is thereby not solved, but merely simplified and carried back a step or two. For just that premise of causal thinking, which Hamilton has uncovered, that in all change some principle of permanence is revealed to us, confronts us again with a synthetic judgment *a priori,* and as such requires explanation and justification. This explanation and justification—like that of our *a priori* insight into the nature of time, as suggested above—remains to be found. We may suspect that these two problems are as closely related as our concepts of time and of change generally, so that, if we knew what time is, we should thereby also know why all occurrence in that time-world must be traceable to some immutable foundation. However this may be, we have reason to hope confidently that these problems, just like those we have already discussed, shall prove amenable to some solution, which shall corroborate the self-reliance of human reason, i.e., shall prove that that which to us appears self-evident is actually not lacking in sufficient reason.

It remains to be pointed out that all the foregoing discussions, in spite of many and important deviations of detail, follow in general the lines prescribed to us by Kant. This is true of the formulation of problems and the method employed as well as of the results. What I have *asked* in every case can hardly be better expressed than in the words of Kant: "There is a pure (mathematics and) science: how is that possible?" In order to find, or at least prepare the way for, an answer to this question, I have everywhere tried to develop the empirical-analytical method, which has been recommended and employed at least by the Kant of the earlier period (in the prize-essay, in the dissertation, in the transcendental aesthetic). And this method has brought me everywhere to conclusions which agree with the Kantian position insofar as they trace back all non-empirical knowledge to some subjective factor or factors, which, in conjunction with the objective ones, place their stamp upon the latter, and thereby make apodeictic statements about empirical data possible.

Ethics[2] allows and requires a treatment similar to that of epistemology, but with a difference. The similarity consists in the fact that, just as theory of knowledge must first collect, order, and relate to fundamental principles the facts of theoretical thinking, so ethics must do the same for the data of moral judgment; but, once this is accomplished, the two sciences are faced not with similar, but with very different further problems. The facts of thinking, as they are given to us, refer to a reality independent of them; every judgment asserts that to the conceptions which it involves some external reality corresponds, and thus in case anything more than our empirical experience of that alleged reality be involved in our conceptions or connections of conceptions requires explanation and justification. Ethical judgments, on the other hand, do not make any assertion about the constitution of objective reality; they merely assert that *if* in that reality there be such-and-such actions, intentions, or characters, these are to be adjudged as good or evil. These problems, then, are not found in ethics; but, conversely, we are here faced with a problem with which epistemologists are not confronted, namely, *the precise conceptual definition of our object of study.* For, whereas we can give an exact account of what we mean, in science and in life, by the word "true," we cannot formulate an equally precise conception of what we mean by "good," although we apply the term with just as much certainty. Thus, once we have established what is to be regarded apodeictically as true or as good in the respective domains, *epistemology has only to discover further how the particular case of truth is to be subsumed under the notion of truth in general, whereas ethics must undertake the task of bringing to light the general notion of "good" which is everywhere presupposed but only obscurely given in the individual case of goodness.* This difference, however, is purely methodological; if the two sciences were complete, they would both start with some immediately self-evident principle and deduce the whole wealth of individual cases therefrom, or else start with the special cases and be able to point out in each one the operation of the fundamental principle.

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[2] Die Methode der Ethik. *Vjsch. f. wiss. Phil.*, 1882, **6**. Zurechnung und Vergeltung. *Vjsch. f. wiss. Phil.*, 1884, **7**, **8**. De wetenschap der zedekunde. *De Gids*, 1899, **63**. Over strafrechtelijke toerekening. *Tijdschr. v. Strafr.*, 1909, **20**. Einführung in die Ethik auf Grundlage der Erfahrung. Leipzig, 1914. Methoden en theorieën op het gebied der ethiek. *Theol. Tijdschr.*, 1917, **51**. De objectiviteitshypothese en de normale instincten. *Tijdschr. v. Zedekunde*, 1920, **1**. Het objectiviteitsbeginsel en de koophandel. *Tijdschr. v. Zedekunde*, 1921, **2**.

So much by way of introduction. In regard to *the objects and conditions of ethical judgment in general,* I have thought it possible to prove that in the last analysis such judgment is never directed toward actions, but always toward the character which is the basis of such actions and may be read out of them upon careful consideration of the existing motivating conceptions; and, furthermore, that it does not presuppose "freedom of the will" in the sense of indeterminism, but rather, on the contrary, presupposes determinism. The former proposition may be derived from the fact that ethical (as well as legal) responsibility is waived always and only in cases where, for some reason (such as physical or psychological compulsion, immature age, ignorance or stupidity, distraction of attention, psychic disorders) it is impossible to infer the individual's character from his actions and motives. The second may be deduced from the first, since such an inference, of course, is possible only if action, motive, and character are interrelated by some natural law. But in individual cases, too, we find that our ethical praise and blame are the more positive, the more the action of the person in question lets us infer a preponderance of higher or lower tendencies in his character; while, on the other hand, in case an action does not fit with everything else we know or imagine we know about the individual, we do not credit it to him as a "free" and undetermined action, but regard it rather as a puzzle, in need of explanation, presuppose some hidden motive, and, for the time being, suspend our moral judgment altogether. If, then, the logical determination of an action by character and motives is recognized in practice not as an obstacle, but as an indispensable condition for moral judgment, the theoretical objections which are raised so persistently against the possibility of ethical judgment and determinism can be explained only as being based on some misconception of the meaning of determinism. And, indeed, it appears upon closer investigation that these doubts all go back to a confusion of determinism as such with one of its special forms, that, for example, they all presuppose that according to the determinist point of view all action is compulsory, or is determined solely by the given motivating ideas, or even by chance associations, but that the willing subject plays either no rôle at all, or at best that of an idle spectator. But once it is understood that this willing personality, by virtue of its own nature which we call character, *endows the motivating ideas with their particular force,* so that one person will attach more weight to personal advantage, another to duty—then it becomes quite clear that determinism, far from excluding the willing personality, on the contrary, must recognize it as the one essential and unchanging factor in the flux of motivations for the determination of action.

A greater difficulty than the recognition of this general relationship is presented by the problem of giving *an exact account of what we really mean by the words "good" and "evil,"* i.e., the discovery of the ultimate criterion which we employ in our moral judgments. For here we have not merely to clear away a few relatively simple misconceptions, but are faced with the task of *deriving a common principle from the multiplicity and variety of moral judgments which occur in various realms, at various times, among various nations, by various persons; a principle which shall have immediate self-evidence* *for any intelligent mind, and from the application of which, under particular conditions and premises, the deductive validity of those judgments may be explained without remainder.* To solve this problem would require an immense amount of psychological, historical, and ethnological material, only a vanishing fragment of which is presently at our disposal; so what I have said about it is nothing but a hypothesis, which, though it has proved successful in a few generalizations performed by naïve human thoughts (the "cardinal virtues"), still stands in urgent need of verification by particular instances. But that hypothesis (which I call the *theory of objectivity*) is to the effect that *a person is always ethically valued in proportion to the degree in which his character shows a tendency to maintain, in all decisions, an attitude of maximal super-individual objectivity,* i.e., to take into consideration all available data and interests in equal measure, without respect to personal wishes or sympathies. Thus, ethics would be, according to Stumpf's apt expression, nothing else than objective-mindedness; and its opposite, the fundamental Evil, would be nothing but egoism, in all its forms and degrees, the limitation of the willing-activity to the self and whatever is relative to it. This theory connects with ancient Oriental and Greek philosophy (Anaximander), with Malebranche and many of the mystics, again, with a long series of philosophers from Socrates to Kant, who felt the presence of some obscure relationship between rational thinking and moral action. If the question be raised against this point of view, what connection there is between the volitional act of sacrificing my advantage for that of another or others, and the faculty of deducing a conclusion from certain premises, the answer in terms of the objectivity-theory is just this: that the validity of the conclusion and the morality of the act are both conditioned by the fact that all available data have been taken into consideration. It remains to be seen whether this theory, as far as we can determine at present, meets our requirements; I think I can answer this question in the affirmative. For, in the first place, I think the conviction that in any conflict the wider, more inclusive point of view as such deserves a preference over the narrower; that consequently particular, individual interests have as little claim against the objective fact in practical action as in thoretical judgment, is indeed not capable of proof, but neither is it in need of any, being self-evident. And, in the second place, in principle at least and with certain reservations and conditions which shall be explained shortly, the cardinal virtues may be deduced from this primitive proposition, as I have said above. As regards *truthfulness,* this may be characterized as *objectivity in respect to things and relations as such;* thus it includes not only veracity in speech, but in thinking as well, i.e., truthfulness toward oneself, and furthermore, conscientiousness in redeeming promises, reliability in money matters, honesty, frankness, and genuineness in every respect. Thus, according to the theory in question, all these virtues owe their ethical value not to their instrumentality toward some extraneous purpose, but possess it in and by themselves, in that they bear witness to a disposition that attributes more weight to motives inspired by the thing itself than to those springing from purely personal interests. And thus, I think, the candid ethical consciousness also judges. Secondly, we deal with *objectivity in regard to fellow men and sentient beings in general,* from which springs the virtue of *benevolence,* of love for one's neighbor *just as for one's self.* Here, it must be owned, the theory makes demands which seem to go considerably beyond those of customary morality, in that it regards a special willingness to serve relatives and friends, benefactors, and compatriots as having indeed a relative value, as preparing the way for a liberation from egoism, but can attribute complete ethical worth only to an extension of this attitude toward all who are in need and deserving of help. But on the one hand, all the great reformers, notably Jesus, have made the same demand; and on the other, there are various complications involved in the cases under consideration (presupposed natural sympathies, avowedly or tacitly assumed duties, better opportunities to render help, etc.) which explain the exceptional position conceded to these cases, and to some degree can even justify it upon the principle of objectivity. And even current morality, though it demands of a moral agent greater sacrifices for his next of kin than for strangers, does not allow him to sacrifice the interests of those strangers, other things being equal, to those of his next kin; fo rinstance, in times of famine to let a foster-child suffer more than his own children, or to use his influence in favor of his friends in filling a position at the expense of other equally qualified candidates. If here we have only an apparent discrepancy, the same may be said in regard to the problem as to why an absolute self-sacrifice, which is as far from strict objectivity in one direction as absolute egoism in the other, has always been granted the highest moral value. In order to find the answer to this question, one must recollect, again, that ethics is not a matter of actions, but of dispositions; and the disposition which is manifested in such self-sacrifice is always objective to the highest degree, be it that in caring for others the agent literally forgets himself, be it that for fear of neglecting others he does not give due weight to his own rights, or be it, finally, that he knows his own failings so much better than those of his fellows, he really feels obliged to prefer their interests to his own. In all these cases we may be dealing with theoretical errors (wherefore an outsider will often be tempted to say: you should think of yourself, too), but these errors bear witness for, not against, the objectivity of the agent's volitional tendencies. Everywhere does the idea of objectivity prove to be the fixed point by which moral judgments are oriented, though they may deviate from it, through adverse circumstances, in one direction or another. Contrary to these personal relations, objective value-differences among the creatures whose interests are involved in our actions may—according to the dicta of the moral consciousness, as well as the theory of objectivity—give rise to differences among our obligations to these creatures respectively; and this is the basis of the virtue called *justice*. Justice demands primarily that equal individuals be equally treated, regardless of the relation which they may bear to the agent—but, likewise, that unequal beings be unequally treated, each according to its deserts: that is to say, in every case *to balance the degree of satisfaction with that of moral value.* That such a balance must be unconditionally demanded of a "moral world-order," is generally conceded; but to determine more precisely the reason and sense of this demand presents some difficulty. I have made the suggestion that it rests at least in part upon the idea that moral wishes in themselves deserve gratification, whereas unethical ones do not; but I am not certain that this notion covers the entire field of retributive justice. However that may be, it is clear that we are here dealing with impersonal motives based upon objective fact, which may claim a higher moral value than those which are relative merely to the ego. The case is not essentially different for the virtue of *chastity*. Objectively considered, the sexual relation is something very great and valuable: a condition not only for self-preservation, but, through selective mating and the rearing of children, also for the physical, intellectual, and moral progress of the human race; it has super-individual significance, and a more or less clear consciousness of this significance is manifested in the profound seriousness which attaches to all normal manifestations of the sexual impulse. Chastity is in essence nothing but the practical recognition of this significance, a respectful attitude toward the sexual relation; wherever it is lacking, i.e., wherever sexual relations are regarded only as a means of individual enjoyment, there we have the same lack of objectivity as in cases of reality-judgments, or acts involving the weal of others, where the objective facts are ignored and only the individual's own interests are borne in mind. Here, as there, of course, it is not a matter of merely observing the particular rules and forms through which public opinion and the state seek to sanction a certain degree of objectivity: one can live in a lewd state of marriage or chaste concubinage, just as one may commit murder for strictly ethical reasons or from sheer egoism become a public benefactor. All that matters to moral judgment is the disposition, and what determines the positive or negative value of such disposition is simply the measure of receptivity for higher, objective, or, respectively, for lower, purely personal motives. Finally, we must consider the so-called *duties toward oneself.* The relation between these and the aforesaid may best be seen through that between the "sovereignty of the moment" and the "sovereignty of the self": just as, macrocosmically speaking, the interests of all men are objectively equivalent, so microcosmically the interests of all moments of the individual life deserve equal treatment; and, as the former may not be sacrificed to the weal of the agent, so the latter may not be sacrificed to the gratification of the present moment. Perhaps one should speak here not of the morality of the total individual, but of that of its separate factors; in any case, a dietetic conduct in the broadest sense has objective value in itself, as opposed to the desires of the moment, and may lay claim to some sort of ethical significance.

My ethics has been misunderstood chiefly in two ways. In the first place, it has been accused of intellectualism; i.e., of demanding of the moral person an exact computation of the opposing claims, or even of reducing morality to an exact knowledge of objective facts. Both accusations are entirely wrong. I have always emphasized the fact that morality does not reside in cognition, but exclusively in volition, not in theoretical, but in practical, breadth of view: whoever in a case of conflict had carefully estimated all interests contrary to his own, and then made his decision in favor of himself, would be a thoroughly immoral person, whereas he who has recognized those foreign interests but vaguely and fragmentarily, but given them as much weight as his own in forming his decision, would be a thoroughly moral person. And the further question, whether the relative value of the competing interests is studiously estimated or intuitively apprehended, though it may be very interesting for the determination of temperaments, is entirely irrelevant for the degree of morality achieved. The second misunderstanding consists in the assumption that, according to the theory of objectivity, an action is ethical only if, in the preceding reflections, the weal of all humanity or even of all sentient beings has been taken into consideration. This, too, is of course erroneous; just as the opinion that in every true judgment the totality of all other truths must be considered would be an error. At every moment of our lives we are faced with a choice among *specific* propositions or *specific* actions; insofar as we conscientiously regard the facts which are relevant to the case, we are satisfying the demand of the principle of objectivity. Thus one who is able to serve only a few persons, or even just one, but does so at the expense of his own interests, may in this way exhibit an objective volitional attitude just as clearly, and merit just as much ethical approbation, as though fate had put him into a position to benefit the whole world. The possibilities among which we may choose are given to us; the choice alone is our own.

Two little essays are all I have published in the way of aesthetics,[3] having given the rest in lectures; yet the subject may claim a modest place in this total presentation, especially upon methodological considerations. For right after epistemology it is especially well suited to illustrate the applicability of the empirico-analytic process in the normative sciences, since it has at its disposal a research material of all degrees of complexity (from the simplest combination of lines to the most elaborate work of art), and it is thereby enabled to abstract empirical laws from the simpler cases and with their aid to explain the more complicated instances. For just this reason it seems to me quite mistaken to limit aesthetics to the artistically beautiful, where almost universally an infinite number of factors in all conceivable combinations contribute to the aesthetic effect, and to neglect the beauties of nature, wherein these factors appear much more frequently in isolation.

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[3] Aesthetische Untersuchungen im Anschluss an die Lippssche Theorie des Komischen. *Zsch. f. Psychol.,* 1896, **11.** Zur Psychologie der Komik. *Zsch. f. Psychol.,* 1899, **20.**

Here too, then, our researches start with the experienced fact that very many and various objects which present themselves to our perception elicit a special reaction of feeling, quite distinct from all others, and whose specific character we express through the fact that to all these objects we give, though with very different degrees of emphasis, the epithet "beautiful." This common resultant element leads us to suspect some common element in the causes (as in the various phenomena called "heat" a common type of physical, mechanical, and chemical action is represented), and we naturally inquire what this common element may be: what, finally, we may accept as the essential character of beauty (as, in the analogous case, we recognize the essential physical character of heat). In order to bring this problem a little nearer to its solution, it seems advisable to make the unwieldy confusion of facts a little more accessible by division of the material; therefore, I have treated separately of the problems of the *content* of perception, the *form* (order in space or time), and, finally, of ideas not given in perception at all, but evoked through association, as contributive elements of the aesthetic experience. I have arrived at the following results.

The separate sensations may be pleasant or unpleasant, but we do not think of them in their own right as beautiful or ugly. Yet they may enhance or disturb the aesthetic effects of other factors: the same tone-sequence will make a different aesthetic impression according to whether it be played in a high or a low range, the same landscape according to whether it be viewed through a red glass or a blue one. This is due to the fact that the different tones or colors express different *characteristic moods;* high tones evoke a cheerful mood, deep tones a serious one, loud and soft tones inspire a feeling of more or less energy, respectively; light and darkness have much the same effects as high and deep tones; in the case of colors, as Goethe and Fechner especially have pointed out, the "positive" ones (yellow, orange, red) produce an increasingly active, excited, aggressive mood, the "negative" ones (various shades of blue) a passive, gentle, melancholy state; the former seem to assault us, the latter to shrink from us, whereas the color green, lying somewhere between these two extremes, effects us soothingly: "one desires to go no further, and one can go no further" (Goethe). In connection with all these effects it should be remarked that, in general, they keep their character even in combinations of various tones or colors; as, for instance, the gradual admixture of red to blue leads us step-wise from a sense of emptiness and loneliness to one of longing (violet), of tragic greatness (carmine), and at last of imposing majesty (crimson). For the present we will restrict our researches to the determination of these facts and express the results in this proposition, that *the aesthetic effects of separate sensations are related to their respective characteristic moods.*

Regarding the *spatio-temporal connections of separate sensations,* we need only to formulate a little more precisely than usual the well-known principle of "unity in plurality" in order to bring all relevant instances under this head. By way of examples of such instances, I would cite first of all the various cases of sense-data repeated at similar space or time intervals: striped or dotted surfaces, patterns of rugs, wall-papers, and brick floors, various designs in architecture and handicrafts; furthermore, chimes, rhyme and alliteration in poetry, rhythm and melody in music; and, finally, rhythmic movements of every sort. Besides these simple cases, there are others wherein not the sense-data themselves, but their mutual relations or their relations to some ideal axis or an ideal focus-point, are repeated (symmetrical or regular figures), or wherein dissimilar factors are made to appear similar through the addition of like elements, or even by reason of mediating relatives to appear less dissimilar (common overtones in musical harmonies, preponderance of white and black in sunlight and gloaming, respectively, direction of the main lines in Gothic or Greek architecture, intermediary stages between lintel and arch in ornamentation, between different colors in painting, etc.). And, finally, it may be that relations of unity obtaining not among the data of perception themselves, but among ideas which are evoked by them through association, prove to be important for the aesthetic impression, examples of which may be found in the effect of images in poetry, the demand for unity of action in the drama and the novel. If we would ask what all these cases have in common, the answer could be only this: the common trait consists in the fact that *in every case the perception of any part is preparatory to the perception of other parts,* in that sense that both, by reason either of their content, their associative connections, or their feeling-tone, are connected by some definite, exact or non-exact rule. Here, too, we will limit ourselves for the present to this formula, and return to it at a later stage.

In the third place, we must inquire to what extent *associations relative to the data of perception* (over and above those unifying relations which we discussed above) contribute to the conditions for the aesthetic reaction. The facts relating to this subject may be classed in two groups, which, according to common usage, I shall denote as the associatively beautiful (in a narrower sense) and the typically beautiful.

Associative beauty in its narrower sense has been brought to the fore particularly through the work of Fechner and Lipps; its general character consists in the fact that a percept is felt to be more or less beautiful according to the kind and number of interesting, emotionally (especially pleasantly) tinged ideas which are associated with it. Such ideas again may occur in two different ways, either as representing personal activities and the feelings expressed in these (*internal association*), or as referring to outward objects which possess some sort of emotional value (*external association*). The former may be found even in simple geometric figures, as when a strongly drawn line reminds us of determined movements, a shaky, weak one of undecided fumbling, when the circle and the sphere seem to us to express energetic tension, the square and the cube appear as pictures of phlegmatic rest, and when we are reminded by vertical lines of strong upward striving and by horizontal ones of weary reclining; furthermore, we experience it in the case of arabesques, which give us a sensation as though the individual lines were seeking, pursuing, fleeing from each other; and, finally, most of all through natural phenomena, all of which we tend to endow with human characteristics, and thereby draw them into the sphere of aesthetic contemplation—as when we speak of proud cliffs, peaceful lakes, yawning abysses, of whispering winds and the howling of the storm, of the thirsting earth, or say that the flowers gaze up at us, open and close their eyes, sleep and wake, hang their heads and faint in the heat of day. The external associations carry somewhat less weight, aesthetically, than the internal; but their influence is by no means negligible, as one may see in the case of scenic beauty, for which adults, who have an infinitely wide range of aesthetic associations at their command, have far more appreciation than children; and, furthermore, the beauty of architectural structures, and useful articles of every description, which certainly depends at least in part on associated ideas, stimulated by the perceptual content, of strength, of economical use of material, practical convenience, etc. Quite wrongly, I think, the latter cases alone have recently been accepted as genuine cases of association, whereas for the former kind other names have been suggested, such as empathy or assimilation. It is true enough that in cases such as I have characterized as "internal association" the additional ideas are not separately given, but fuse with the perceptual content; but that is frequently the case with external associations as well (an "intimate" room, a "liveable" looking house), whereas, in one instance as in the other, the percept is prior to any ideas that may adhere to it as a result of former experiential connections, whether these be separately recognizable or interpreted directly into the percept. The only distinction I can see between internal and external associations is that in the former case the connection of the given percept with the resultant feeling is effected through an idea of expressive actions, which may be treated as results of the feeling, and the latter, through ideas of external characters, which may be treated as causes thereof. If we compare all these cases with other types which also stimulate pleasant ideas, without, however, having any specifically aesthetic import (tasty looking dishes, agreeable news), we shall presently arrive at the conclusion *that emotional associations are especially fitted to evoke an intense aesthetic reaction if, by reason of their great number and indefinite character, they rise barely, if at all, over the threshold of consciousness.*

Finally, we must consider the typically beautiful, in which, as we shall find, association also plays a certain part, though in a very different way and quite regardless of any feeling-tone. When we admire a horse, a thoroughbred dog, a human body as perfect after its kind, we obviously are comparing it somehow with an ideal image which is contained in our mind; the question is how we arrive at these ideal images. For the solution of this problem, a resort to sensuous, formal, and associative beauties belonging to these standards is certainly very inadequate; and even the very popular word "idea," which, except in the Platonic universe of discourse, is nothing more than a word and therefore should be eliminated as soon as possible from scientific vocabulary, does not get us one step further. But when we inquire directly of experience, we find *that these ideal images, which we use unconsciously as norms in judging various specimens of a genus, conform, all in all, precisely to the average which we have derived throughout our life from all our experiences of the genus in question.* Direct corroborations of this may be seen in so-called "type-photography," and the measurements which have been taken of Greek sculptures; indirectly, it is borne out by the variations of the ideal of human beauty among various races and nations, the gradual mutation which such an ideal undergoes in case of protracted sojourn among other peoples, our general aesthetic horror of monsters and intermediate forms (such as the bat), the influence of prevailing fashions, which appear always first ugly, then tolerable, and finally so natural that their displacement by some other style leaves us definitely with a sense of something lacking; and many other phenomena. Even in the domain of art we can trace the same factor; for not only do we recognize joyously in old architectural works certain relatively casual details just because they happen to belong to the style of the period, but also our modern genre art, just like the Greek plastic arts, achieves its aesthetic effect preferably through the presentation of general types. Therefore, we may safely conclude (here, as always, in default of disturbing circumstances) *that the perception of forms which correspond to the average of the perceiver's previous experiences is accounted beautiful.*

Thus we have set up, at least in their most general outline, four empirical laws, each of which tells us that under certain special conditions the specific feeling of beauty-perception appears in consciousness. It remains to be determined whether those four laws of special conditions (of sensuous, formal, associative, and typical beauty, respectively) have a common factor, which would explain the common element in their effects.

Now I believe I can find this common element through the fact that *in all relevant cases there are certain conditions which bring about a constant or ever-recurring adjustment of attention to the percept.* This is most easily proved for the case of formal beauty, whereby the regular alteration of perceptual contents (as in the sliding of one's glance over a pattern, or in hearing a rhythmic tone-progression) associations are soon established among these contents, which let us expect at every minute just that which is about to appear; or where (as in the reading of a drama or a lyric poem) we are presented from the very beginning with notions of characters and situations or put into a mood into which everything that follows fits precisely, despite all variations of content. The case of the typically beautiful is very similar, but with this difference, that here the associations which bring about the concentration of attention are not produced at the time of the particular perception, but have been previously established. Those previous perceptions have combined the common elements and the *average* individual traits of the specimens of a group into a more or less conscious general notion, which makes the apprehension of any new specimen easier or harder according to the degree in which the specimen conforms to this notion. With *associative beauty* the case is somewhat different, but ultimately really the same. If a perceptual content calls forth interesting and emotionally tinged associations, these will persist in consciousness just because of their emotional import; if, however, they are numerous and not very clear, they will inhibit each other and thus remain in the background, but from there they will react associatively on the content and thus keep the attention focussed on it. Hence the spell by which an object of associative beauty (a dreamy landscape, an expressive face) binds us, the sense of infinity which it inspires, in sharp contrast to the effect of formal or typical beauty. Hence, also, the lack of such an effect in cases where the emotional associations evoked are less numerous and more definite (practical art-objects), because in such instances the associations claim attention for themselves, and, instead of supporting the perceptual content, enter into competition with it and crowd it out of the focus of consciousness. As for *sensuous beauty,* it should, in general, be subordinated to the associative sort. There is such a far-reaching parallelism between the emotional character of tones and colors and their most notable exemplifications in the experience of the individual or the race (intonations of the human voice at various ages and in various states of mind, contrast between day and night in respect to safety and to power of attention, red color of fire and of blood, blue of the sky and of the calm sea, green of surrounding nature) that we cannot help suspecting some real, and yet, fundamentally, associatively determined connection. But, in view of the slight significance of the sensuous factor in aesthetics, it is hardly necessary to dwell upon this point.

But if it is a fact that wherever aesthetic pleasure occurs there are also conditions in the object of perception which either in themselves or in conjunction with earlier experiences focus the attention on the content in question, then we seem to be justified in our assumption *that from the feeling, thus created, of alleviated functioning, of effortless apprehension, of thorough-going harmony between the function and the object of perception, springs that specific pleasure which we denote as aesthetic pleasure.* And, indeed, we find that wherever—even though it be for casual and purely individual reasons—such a sense of alleviation of perception occurs, there occurs also a sensation of pleasure which is unmistakably related to aesthetic pleasure: as when, for instance, after long wandering in foreign places, we first catch sight again of some familiar spot, or hear our mother tongue, or even when a disturbing noise ceases, or the fog which obscured an interesting view clears away, or an image under the microscope or on the screen suddenly, by a change of focus, acquires definite outlines. In all these cases we experience a feeling of release and liberation just as when we meet with a beautiful object, and the only reason why we do not denote our percepts as beautiful is that we know the effect to be merely transient, due only to contrast with what went before.

A further corroboration of the theory here outlined lies in the fact that it does justice to the agreements and differences between the genuinely beautiful and its variants (the sublime, the tragic, the comic). The sublime causes us to vacillate between two moods, in that we alternately regard the great, eternal, overpowering or perfect object in opposition to ourselves, thus realizing painfully our own inferiority, and empathetically identify ourselves with it, forget ourselves, and rise with it above the human sphere. The transition from the former to the latter state must always carry with it a feeling of liberation and relieved tension, but in much greater measure than the above-mentioned internal associations; which explains without further difficulty the aesthetic appeal of the sublime. Of course, these feelings are sometimes replaced by their opposites, which causes that feeling of displeasure to which the sublime, in comparison with the purely beautiful, owes its greater degree of seriousness; but in active, strong, imaginative natures this feeling remains in the background and serves only to enhance the other by presenting a contrast. And of weak, timid, prosaic natures it is actually true that a sublime drama, which delights stronger souls, depresses these. The reason for our appreciation of *tragedy* has been sought in all sorts of extra-aesthetic factors (catharsis, poetic justice, moral greatness of the hero, fate or causal necessity), every one of which occurs frequently, but none unfailingly, nor only in cases where the tragic impression is manifest. But there is one, and, as far as I can determine, only one, element which is never absent in a tragedy or tragic situation, and which according to the present theory must necessarily have a profound aesthetic import: namely, *the intense fixation of attention upon a great sorrow.* For, as daily experience teaches us, although small sensations of displeasure attending any percept have a repelling effect, and consequently hinder protracted perception and cause the percept to appear ugly, great ones concentrate our gaze with irresistible force upon the object which provokes them, and despite our horror, or rather just by reason of it, make it impossible for us to turn it away. But thereby a degree of concentration of attention is produced which in and by itself should produce an intense feeling of aesthetic pleasure, provided there are no conflicts involved. Such conflicts, however, are involved, namely, conflicts with those feelings of displeasure which by their concentration of the attention condition the aesthetic pleasure, but in themselves, of course, keep their peculiar feeling-tone. So we are not dealing here, as in the case of the sublime, with a swift alternation of pleasure and displeasure, but find that, exactly as often and as long as the extra-aesthetic displeasure, the aesthetic pleasure is given, and which of them is to have and retain the upper hand depends entirely on their relative intensities. But this balance of intensities varies with different persons and conditions. A bloody battle-scene, a horrible accident will inspire only very crude and insensitive people with more pleasure than displeasure; the presentation of similar things in pictures or on the stage is immediately less productive of displeasure, and may yield an excess of pleasure over displeasure on a much higher level of culture; but a person of very superior culture requires far more effective aids and compensations in order to feel the pleasure of effortless perception resulting from displeasures really as such. And this is just what tragedy offers him in all those aspects of satisfaction of his sense of justice, moral admiration, insight into pure necessity, each of which is often wrongly taken to be the positive and essential basis of tragic enjoyment. Wherefore any one of these aspects may be lacking in a good tragedy, but never all of them at once. As for the *comic,* finally, I have in the main, though with slight deviations, followed the theory of Lipps, according to which, in every case where the effect in question is achieved, *attention is first keyed up* (by an apparently significant or new or puzzling content), *then suddenly let down* (as by the sudden realization that the apparently significant item is nonsensical, the novelty uninteresting, the puzzle perfectly clear). For the factual foundation of this proposition I would refer the reader to the works of Lipps,[4] and to my articles which are mentioned above; here I would only remark that it applies without exception to the objectively funny (the ludicrous), the subjectively funny (the witty), as well as to many other phenomena which are frequently overlooked (such as the laughter of children in playing tag or in being tickled, of young girls in a rocking row-boat, or nervous laughter under great physical or mental pain). If this is correct, then the relation between the comic and the beautiful is easily explained. In the one case as in the other, the resultant pleasure-sensation arises from a release of attention, which carries with it the pleasurable feeling of an increase in available power of attention; the chief difference is this, *that in comic situations the preceding tension, by reason of the special conditions described above, is more intense, and that in the second place the release is effected not by constant additions of related items which support the perception, but rather by the sudden falling away of interest in the perception.* This explains the convulsive character which belongs to comic sensations in contradistinction from purely aesthetic ones, and which is mirrored adequately in the corresponding expressive movements, namely, laughter, where a deep inhalation, symptomatic of the tension, is followed by a spasmodic exhalation that signifies the release. An obvious simile whereby I used to illustrate this relationship seems to me still in order: a heavily weighted spring may be released in two ways: either in that the weight is supported from below, or in that it is torn off and falls to the ground. The former case corresponds to the aesthetic, the latter to the comic sensation.

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[4] Lipps, Th. Psychologie der Komik. *Phil. Monatsch.,* 24, 25. Komik und Humor. Hamburg u. Leipzig, 1898.

The field of application for aesthetics is the field of the fine arts: not, of course, in the sense that the artist should study aesthetics and let the results of such research determine his art, but rather, in this sense, that the aesthetic laws apply themselves, and are subsequently rediscovered in the finished work of art by the student of aesthetics. What it comes to is this: the artist, by virtue of his strong interest and his susceptibility to aesthetic impressions, conceives his work, be it through free imagination, be it through some feeble suggestion from an existing reality; but in that his conception satisfies him and its execution pleases others, those very laws are manifested which everywhere underlie the recognition of beauty. Consequently, in every good work of art we will be able to discern, in all possible nuances and variations, elements of sensuous, formal, associative, and typical beauty, and perhaps other kinds as well. It would be highly interesting to examine from this point of view the various kinds, periods, and tendencies of art, to determine for each the predominance of some particular factor or factors, and certain modifications of the same, and trace their connection with the demands of technique, the spirit of the time, and the character of the people under consideration. But this task the student of aesthetics may leave to the historian of art.

The application of the empirical method to *metaphysics* scarcely requires any elaborate justification.[5] For, in the first place, it has been recognized at least implicitly that a science of reality, such as metaphysics purports to be, should orient itself by the phenomena through which we receive knowledge of that reality, and use them to test its results. And, in the second place, the empirical sciences, in trying to adapt their hypotheses to a greater and greater number of facts, demand to be crowned with an empirical metaphysics. Just as the hypotheses of physics require complementation or modification if the phenomena of chemical as well as physical phenomena are to be taken into consideration, so do the hypotheses of general natural science, if we want to explain not only those phenomena which it has usually accepted, but also facts of consciousness, and the relations between the two sorts of phenomena. And thus a general science of the world, which examines what our necessary presuppositions must be if we are to take account of the totality of available data of experience, is absolutely in line with historical and logical development.

Thus the first question which metaphysics must raise concerns the kind and the law of those facts which are at its command for the construction and the final checking-up of its hypotheses. In regard to the *kind* of facts it deals with, a simple process of reflection will show us *that they are, one and all, facts of consciousness:* that is to say, sensations, impressions, ideations, judgments, feelings, volitions, etc. This could not be otherwise, for if there were anything of which we were *not* conscious, it could not serve us as a datum for the construction of our world-outlook. Of the *natural law* which is manifested in these facts of consciousness we will have to speak at somewhat greater length. In part, this law obtains among the given contents of consciousness themselves, as, for instance, between perceptions and their related feelings, ideations, and thoughts, between premises and the conclusions which follow from them, between motives and their resultant volitional decisions; these are the subject of psychological research. For the remainder, however, the given facts of consciousness indicate a governing principle which lies not within but without their own sphere; this is a subject for the natural sciences. Two sets of facts are immediately relevant to the discovery of this principle. In the first place, the circumstance that *for a certain class of conscious phenomena, namely, the sensations and perceptions, no uniformly associated antecedents can be found in consciousness,* so that a phenomena of this sort, e.g., a sound, a smell, or a sight can appear in consciousness although nothing has occurred there just previously that has any definite and uniform connection with it. And, in the second place, the fact that *under favorable circumstances* (as with continuous adaptation of the sense-organs, in the broadest sense) *those sensations and perceptions occur in sequences which follow a regular course, and which are not thrown out of their course through the omission of some of the items by reason of occasional interruptions of the necessary conditions* (as when one shuts the eyes or turns them away). This circumstance is most readily explained on the assumption (originally made in pre-scientific thought) that besides the data of consciousness there is something else in existence, an "external world," governed by strict law, and whose parts, through the mediation of the sense-organs, are able to produce impressions and perceptions in consciousness, in which their regularities are mirrored. But what these parts, or the external world in itself, may be apart from the effects upon consciousness, that we cannot guess at all from those effects, i.e., from our sensations and perceptions—not even that such an external world is spatial or in space. For, even in our spatial perceptions, we are certainly dealing with very mediate effects of the real conditions, and even the mediators of that relationship (the sensory processes) are known to us only through their very remote effects; therefore, just as we may not infer from our sensations of redness or sweetness that the external objects which cause these sensations are themselves red or sweet, we have no right to suppose that because we perceive extension and motion these must exist as such in the external world. All that natural sciences teach or can teach us concerns merely the uniform connections among absolutely unknown things and processes which can be characterized merely by virtue of their respective sensory effects. And this holds not only for empirical laws, but likewise for explanatory hypotheses; but with this difference, that in the latter case (e.g., in propositions concerning thermodynamics or Bohr's model atom) those things and processes are characterized not by the perceptions which they produce in a human observer, but by such as they might produce in an ideal observer with similar but infinitely refined sense-organs at his disposal. In short, natural science offers us a very comprehensive, very precise, but strictly *relative* knowledge of external reality; it tells us how this reality, under ideally favorable circumstances, would *appear* to us, but not what it *is* in its own right.

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[5] Zur Parallelismusfrage. *Zsch. f. Psychol.,* 1897, **17**. Einführung in die Metaphysik auf Grundlage der Erfahrung. Leipzig, 1905. (3 Aufl., 1921.) Wetenschappelijke Metaphysica. *De Gids,* 1906, **70**. Het ik en't psychisch monisme. *Tijdschr. v. Wijsb.,* 1907, **1**. De philosophie von Henri Bergson. *Tijdschr. v. Wijsb.,* 1912, **6**. In Sachen des psychischen Monismus. *Zsch. f. Psychol.,* **63-79**; 1. Missverständnisse in Bezug auf die metaphysischen und naturwissenschaftlichen Voraussetzungen des psychischen Monismus (1912, **63**); 2. Missverständnisse in Bezug auf die psychologischen Voraussetzungen des psychischen Monismus (1912, **63**); 3. Psychischer Monismus und Psychical Research (1912, **64**); 4. Die Beziehung der Wahrnehmung auf ihren Gegenstand (1916, **75**); 5. Leben und Traum (1916, **75**); 6. Dualistischer und monistischer Psychismus (1916, **76**); 7. Die neuesten Bedenken Erich Bechers (1917, **79**). Spinozistisch en modern parallelisme. *Bijdr. Kon. Acad. v. Wet.,* 1914. Het psychisch monisme. Baarn, 1915. Über die Anwendbarkeit des Energiebegriffes in der Psychologie. Leipzig, 1921.

Over and above the psychical and the physical uniformities, however, we find a third type, namely *psychophysical law.* Among the realities concerning which natural science gives us a constantly increasing relative knowledge we must count also those which we ideally might perceive as functioning brains; and, on the evidence of numerous anatomical, physiological and psycho-pathological researches, we may assume that these perceptions "run parallel" to the contemporaneous contents of consciousness of the person whose brain is under consideration, i.e., that they stand in consistent causal relationship with those contents. Thus, if someone were thinking, for instance, about a problem over a certain period of time, or indulging in day-dreams, without interruptions by outer stimuli, it could be shown that every occurrent content of consciousness is determined by previous contents and every concomitant brain-process by preceding brain-processes, while, on the other hand, every content appears to be uniquely determined by its accompanying brain-process and vice versa. This peculiar interlacing of the various relations of dependence can hardly be given any other interpretation than this, *that the two series of phenomena arise from one common source, one reality which may be perceived in two ways* (as for instance a vibrating string may be perceived through the eye as well as through the ear), and this assumption actually serves as the foundation of three otherwise very diverse cosmic hypotheses. *Materialism* maintains that this reality is ultimately *matter,* which only produces the false illusion of consciousness in cases of a certain highly complex combination of particles which produces what we call a nervous system. *Psychical Monism,* on the other hand, conceives Reality as a whole as a huge complex of *conscious processes,* which splits up into a plurality of individual consciousnesses, and within which under special conditions (these being, again, perceptible as functioning sense-organs) the particular conscious contents arise that we designate as material (possibly as cerebral) phenomena. *Spinozism,* finally, presupposes a *Reality unknowable in its ultimate essence,* whose parts may be perceived, on the one hand, as material things, on the other, as conscious processes. Each one of these hypotheses is as well fitted as any of the others to explain the threefold world-order, since there is for each one of them a connected and ordered reality which is mirrored more or less perfectly in one or more series of phenomena, and which, by virtue of this mirage, must express its own essential order. Thus, for materialism the causal order of cerebral processes is reflected in the sequence of concomitant phenomena of consciousness, for psychical monism the law of human, animal, and other conscious life is mirrored in the brain processes and other perceptible phenomena which accompany it under favorable conditions, and for Spinozism the law of the "unknown third term" is reflected in the other two orders. But, if we inquire which of these three makes the fewest assumptions and can attain the greatest results, it seems to me indubitable that on both counts we must give the preference to psychical monism.

In regard to the first item, *the minimal number of assumptions,* this seems to me obvious. While immediate experience gives us, as we remarked above, nothing but contents of consciousness, which include sense-perceptions, psychical monism never assumes any other kind of reality, but presupposes merely that within the scope of some greater consciousness-pattern these sensory percepts are generated by causes of essentially the same nature as the immediately given contents of consciousness. *And in the assumption of this causal connectedness lies the only hypothetical aspect, the only element not* *directly taken from experience, of the monistic theory;* whereas, for the rest, it complements experience—as any theory must do—only by the assumptions of further elements of just the same kind as experience has presented. Materialism, on the other hand, assumes, besides the given contents of consciousness, a totally different sort of thing, a non-given matter, and Spinozism presupposes an utterly unknowable third Something, and both are forced to assume new types of activity to be assigned to the new essences; wherefore the increase of assumed elements is not compensated for in any way by a decrease of assumed functions. Thus, the greater simplicity of psychical monism can really not be questioned.

Furthermore, how do the competing hypotheses rank in regard to *their potency to explain the phenomena in question?* As we remarked above, they are all capable, in principle, of accounting for the existence of a physical, a psychical, and a psychophysical causal order; but that is not saying that they can equally well do justice to the particular contents of these three orders. And, indeed, in this respect they show important differences, of which we will treat in a little more detail.

In the first place, *materialism* cannot do justice to *the proper essence of that which we experience as the content of consciousness.* The fundamental hypotheses and laws of natural science, which the materialist claims to find sufficient for the explanation of consciousness, deal only with the generation of motions from other motions: that the motions of infinitesimal brain particles should give rise not only to other motions, but also to consciousness, is not provided for in any part of the doctrine. If, then, the materialist would fill this gap by endowing matter not only with mechanical but also with some further hidden powers, which might account for the genesis of consciousness, he runs up against the law of the conservation of energy, since he must suppose that this process as for any other activity of material agents some energy is expended, whereas we know, from the general assumptions of natural science as well as from special researches, that the energy received by a body is spent without remainder upon its physical activities. If, then, the materialist would surmount these obstacles by simply assuming that somehow and somewhere consciousness has arisen from mechanical sources, he is faced with the difficult problem of *the persistence and growth of consciousness,* since Darwin has taught us that only those organic functions persist which have survival value, but consciousness, for the materialist, is a mere epiphenomenon and therefore entirely ineffectual. In every way it is evident that the materialistic hypothesis is essentially unfitted to explain the facts of consciousness, and can never produce an account of them without violating those very assumptions of science which are its own foundation.

Spinozism (especially in the simplified form presented above) is safe-guarded against similar reproaches simply by the fact that it tells us absolutely nothing about its supposed ultimate Reality, wherefore no one can say what the latter might or might not be capable of doing. Yet there is one general fact which this philosophy must regard as absolutely accidental and inexplicable: that is *the contextual agreement of volition and action.* For, according to Spinoza's doctrine, there is no causal relationship between these two phenomena, by virtue of which the decision of the will could produce an appropriate action; but each reflects in its own way a real process whose nature is unknown, but which must certainly be conceived as quite different from either of its perceptual expressions. Thus, it is a matter of pure accident as regards the physical image (action) that there should be a psychical image (volition), and doubly accidental that in this volition just those motions are conceived as ends which in fact are just about to occur in the action. Thus Spinozism (as also, by the way, the materialistic doctrine) fails to solve that question of supreme importance for all our further problems, why we act in accordance with our desire; for the explanation of this fact it would probably require supplementary hypotheses, and is therefore obviously inferior to a philosophy which can get along without such aids.

Now I believe it is easy to demonstrate that *Psychical Monism,* for all that it begins with fewer presuppositions, can accomplish more than any of its rival doctrines on this point and at least as much in any other respect. In the first place, it assumes nothing save an immeasurable totality of conscious contents, which includes the sense-perceptions of men and animals; which latter, however, are not produced by hypothetical material substances, but depend in their turn upon further contents of consciousness. Thus, psychical monism need not assume and explain the origin of matter out of mind, as materialism must account for the genesis of mind out of matter. The law of the conservation of energy, too, can here be accepted without reservation. If all Reality is consciousness, and if the physical objects we perceive are merely reflections in our consciousness of some other consciousness, then that which we measure as physical energy must itself pertain in the last analysis to the psychical Reality; in this itself there must be some invariable magnitude which is revealed in the world of phenomena as energy, kinetic, potential, or thermo-dynamic, and so forth. If we were blessed with a perfected natural science and a perfected system of psychology, then there should be two ways of measuring this psychical energy: either directly by the processes of consciousness themselves, or indirectly by the natural phenomena which are the appearances of these processes; the real object of measurement would, however, be the same in either case, and the fact that it is conserved without any increase or decrease should be demonstrable by the results of either the direct or the indirect system of measurement. Furthermore, the effect of the struggle for existence and the survival of the fittest may be interpreted in psychical as well as in physical terms, since indeed our ideas, our views, our volitions are always battling for supremacy and can attain it only insofar as they are especially well suited to our total personality, or at least to its present phase. Something analogous to this, although on a very different scale and presumably upon a different level of evolution, might lie behind that which we call the struggle for existence. And, finally, psychical monism has the tremendous advantage over its rival hypotheses that it does not deny the causal efficacy of the will, but rather presupposes it. For a decision of the will, according to this philosophy, belongs not to the realm of ineffectual phenomena, but to that of effectual reality; it causes further real and effective processes, which are known to us only through their accompanying phenomena (nerve-impulses, muscular contractions, actions); and its effectiveness is guaranteed by experiences which we have collected throughout our whole lives, concerning our ability to produce ideas of motion, and concerning the relation between these ideas, on the one hand, and the perceptible motions and their consequences, on the other. Thus, psychical monism appears adequate on every count to offer simple and certain answers to just those problems which other cosmic hypotheses necessarily leave open. And to my knowledge there are no other difficulties which are peculiar to this doctrine but do not beset one or all of the others.

It remains only to indicate briefly the general lines upon which the fundamental idea of psychical monism may be further developed. All that is actually given to us in experience of the cosmic consciousness which the doctrine presupposes is the individual human and animal consciousnesses, and *directly* we are given even just one of these, namely, our own; thus we must begin with the latter if we would reach any conclusion as to the cosmic consciousness. Now we know that among the countless sensations, memories, cognitions, etc., which compose our mental content there can be but a very few at a time that "rise above the threshold" and thus stand forth from the rest in relative independence, and experience of dreams and of multiple personality has taught us that there may be several such complexes in existence simultaneously. It is not far-fetched, then, to suppose that the differentiation of the individual from the cosmic consciousness is simply analogous on a large scale to the process that actually takes place in these instances in miniature. Also, it looks as though in both cases the same determining conditions were to be found. For, just as a central content in the individual mind, quite distinct from all other contents, is determined by the help of particular sensations or ideas which, by reason of their emotional coloring or their intensity, have reached a high degree of consciousness and thus attract some associated ideas and sensations and exclude others—so there are in the cosmic consciousness innumerable systems of associated organic and other sensations, each such system having reference to one body, being uninterrupted in its continuity, and accompanied by satellite images, thoughts, and feelings; and such a system may well suffice to distinguish us from cosmic consciousness during our individual life just as much as the objects of our own passing interests are distinguished for us from the rest of our spiritual content. This analogy could then be carried out in height and in depth without limit. As our total mental content is reflected in the appearance of a functioning brain, and our central focus of consciousness in a certain pattern of brain-functions, just so we might find in the phenomena of earth, solar system, and milky way, on the one hand, and of cell, molecule, and electron, on the other, the sensible images of more or less inclusive concentrations of consciousness. But all these separate consciousnesses would have to be conceived as being in their turn integral elements of higher ones and ultimately of the world consciousness, all bound together by one supreme law. Certainly, all these adumbrations are still very far from yielding any sure scientific results. But the constant progress of science and psychology will enable us to put them to more and more conclusive tests. The more we learn about the parallel relations among the data of consciousness and their bifurcations, on the one hand, the accompanying brain-processes, on the other, the more precisely we shall be able to trace those phenomena in the external world which point to similar processes and relationships; and the more we shall come to know about the natural history of the earth and the celestial systems, molecules, and atoms, the more exactly shall we be able to appreciate the extent of the analogy which Fechner and Spencer demonstrated as holding between this and the natural history of living organisms.

If, finally, the question be raised, what sort of reality is to be attributed to the cosmic consciousness postulated by psychical monism, the answer must be: the same that pertains to any given human consciousness. But if, then, the question be pressed as to whether this cosmic consciousness must be taken to exist in its own right, or to be itself a mere phenomenon, I would say that I cannot, like Kant and his adherents, attach any absolute significance to this question. Rather does it seem to me *that the concept of "phenomenon" is a purely relative one;* we call a thing a phenomenon, or appearance, not with reference to its own nature, but to its relation with something else which acts as its necessary condition; but a thing which is a "mere phenomenon" of something else is yet something "in itself," and for the duration of its existence belongs truly to Reality, in the same sense as the other. This conception leads us to assume something like a series of levels of Reality, each of which is to the next lowest as an appearance to its underlying cause, and in relation to the next highest plays the part of "thing in itself." The uppermost stratum is that of sensations; then comes the level of those things which give rise to the sensations, and which, according to psychical monism, are one and all facts of consciousness. Insofar as these two are given directly to our experience, they certainly are part of Reality; but insofar as we add intellectually further, non-given elements, the case is a little different for each of these two instances. In regard to the sensations we find that they are produced singly through the mediation of what we perceive as functioning sense-organs; wherever, then, the continuity of their orderly sequence is broken, we have no reason to postulate non-given sensations to fill the gaps, but merely such realities as might produce sensations under the proper organic conditions. With respect to all other types of conscious contents, however, we lack any justification for supposing them to be conditioned by anything else than further facts of consciousness; in cases where their causal connection is broken, therefore, it certainly is simplest to assume the existence of non-given contents of consciousness by way of supplementation. And this view is borne out by the fact that a series of continuous sensations, if interrupted by temporary suspension of the organic functions involved, will *always* continue, when these are renewed, as though there had been no break, whereas a sequence of any other sort of conscious contents, if somehow interrupted, will *never* do so. Consequently, the given sensations must not be conceived as part of an inclusive real complex of sensations, but the given consciousness should be considered as part of an all-inclusive real complex of psychical contents; and this suffices to explain in principle both the general regularity of psychical phenomena as well as the occasional breaches in this regularity, which let us perceive, on the one hand, the individuality of sensations, on the other, the totality of conscious life. But, if after this we should feel inclined to suspect, as Kant did, on the strength of our *a priori* knowledge of the temporal and causal truths which hold for consciousness, that there be even a deeper, non-temporal level of Reality, which supplies the logical ground for these *a priori* facts, this does not in any way challenge the reality of our temporal and causal world of consciousness, but merely asserts the possibility or probability that the latter is not all of Reality. Also, there would be no intelligible meaning in the assertion that this suspected third stratum possessed a higher or in any way different sort of reality from the first and second; for the general predicate or Reality does not allow of differences in degree or kind, as do the properties of particular real objects. And thus we may safely assume that psychical monism, if it shows us only a part of Reality, shows us at least that part which is of highest importance for us, lets us recognize it as it is in its true nature, and will continue to let us recognize it more and more fully and exactly.

As for personal details that might interest the reader, I have practically none to add here. I was born in 1857 at Ferwerd in Frisia, attended the scientific high school (*Realgymnasium*) at Leeuwarden, graduated from there, studied political science and philosophy (the latter under Land and Windelband) at Leiden and at Freiburg in Breisgau, and since 1890 have been active at the University of Groningen as Professor of Philosophy and Psychology. To the *Realgymnasium* I owe my profound respect for the objectivity and strictness of scientific proof; political science was no more than a passing interest with me, from Land and Windelband I learned a great deal and received much inspiration. Among earlier and contemporary philosophers, it was most especially the younger Kant and Fechner, as also Hume, Lipps, Riehl, and Sidgwick who guided me in the way I was to go. For the rest, I have taken the problems as I found them, and by old and tried methods (only too rarely practiced in philosophy with clear comprehension and consistency) I have sought to find or at least to approach their solution.

HARALD HÖFFDING

My interest in psychology awakened early. Apart from the reflections which life gives rise to in a young man, I was led partly through the study of Plato and partly through the problems of religious emotion to speculate on the mind of man. Later, the momentous part played by Kierkegaard in the spiritual life of Denmark could not but act as an incitement to test the sincerity of the faith people imagined they held. In college I learned about the descriptive and biologically influenced psychology of Sibbern. It became of lasting importance to me that Sibbern firmly maintained the independence of psychology, it being part of his teaching that, just as the botanist does not recognize weeds as such, but studies them like all other plants, so the psychologist does not distinguish between Good and Evil, Beauty and Ugliness, but tries to recognize all forms and tendencies within the human mind and to discover the laws according to which they develop. More special tasks were set me by a discussion which at that time was being carried on in the literary life of Denmark as to the mutual relations of science and religion. I was led to inquire into the relative positions of these two spiritual forces within the realm of human thought, into their psychological basis, and into their various forms. Besides psychology, the history both of science and of religion became of great importance. As the problem presented itself to me, the epistemological side of it had to prevail. All the lines indicated here were determining factors for the development and tendency of the study of philosophy to which I have devoted my life.

When I began to make a special study of psychology (about the year 1874), this science had just reached its earliest experimental stage. Fechner, with his psychophysics, and Wundt, with his physiological psychology, were my first teachers in this field. I have learned much from experimental psychology, but there are many mental phenomena on which it does not throw any light. This has also been my attitude towards behaviorism and sociological psychology (the latter I have dealt with in a paper on Durkheim's masterpiece in the

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[*] Submitted in Danish and translated for the Clark University Press by Professor Aage Brusendorff of the Department of Scandinavian of the University of Minnesota.

[†] Died July 2, 1931, at the age of eighty-eight years.

Revue de Métaphysique et de Morale, 1913). The results obtained by all these methods must be interpreted directly or by way of analogy through the efforts of introspection. The study of psychiatry has been of great importance to me, as cases of mental break-up testify to the important part played by energy and concentration in keeping up the continuity of mental life. When, later, a French edition of my *Psychology* came out, I was especially pleased to see that Pierre Janet had written the preface.

My *Psychology* appeared in Danish in 1882 at a very opportune time. Men were beginning to feel drawn towards the study of mental phenomena by way of reaction against the speculative and scientific interests that had prevailed for the greater part of the century. But at the same time they were becoming aware of the immense variety of sources which psychology ought to make use of and which ought to center on the direct observation of oneself and others. I knew how great the task was, and, had I then been aware of the trend of future developments, I should hardly have had the courage to begin my book. The many readers it got throughout the world seemed, however, to show that a book of that kind was needed. Even though its time may be over, I believe it to have expressed ideas that, perhaps in a different form, may still hold a message for the future.

My work on psychology is composed in such a way that, first, on the basis of certain functions of the mind, a hypothesis is attempted, which, then, in a special part of the volume, I try to make out in particulars. The functions on which I have placed special stress are memory, comparison, pain, grief, disappointment, doubt, concentration on a purpose, and search for means to carry it out. The common type I found to be a synthetic effort and a mutual relation between mental phenomena, determined by this synthesis. Spontaneous synthesis became to me one of the basic facts of psychology. In science, art, and everyday action we may then do further work with more or less conscious synthesis, but the type remains the same. And spontaneous synthesis may, through analysis, lead to psychological understanding. Making use of the various sources of psychology, indicated above, I try then to prove the truth of the attempted hypothesis in particular instances. I take my starting-point in the usual tripartition of the psychical elements, and set out to prove that, owing to the law of synthesis, mental energy becomes the central conception of psychology. If any one of the three notions employed by the usual division were to be made the basic one of psychology and be indicated as the central fact of mental life, it must be the will (in the widest sense of this term). The entire growth of the human being proceeds from one voluntary process to another, and psychology as a whole might be fully accounted for as a psychology of the will, the phenomena of thought and emotion being made conditions or effects of volition (in the widest sense of the word). In a short paper (in French in the *Revue de Métaphysique et de Morale,* 1907) I have tried to define the conception of volition in the widest sense of the term as employed by me.

This view conforms to our knowledge of the structure and function of the nervous system, especially as to the relations between 'higher' and 'lower' centers established by physiology: the 'higher' centers exercise a synthetic and concentrating influence over the 'lower.' The relation between psychical and physical functions may be conceived of as parallel to the relation between tones and notes in music or between mathematical equations and successive phases. From here it is not a far cry to Spinoza's conception of things spiritual and material as 'complementary' phenomena. The intellectual attempt at establishing such 'complementary' relations constitutes our last and lasting problem.

It was of no little importance for my teaching of various philosophical disciplines at the university that I had adopted a thoroughly psychological view of the human mind. Here was a field open to everybody. It is only in the course of special developments of the human mind that intellectual, ethical, religious, and aesthetic differences clash, and it may then be a good thing to trace the way back to the stages where the contrasts had not yet appeared. My work on psychology also aimed at showing the starting-points for the various directions of the human mind. In this way, the book might be used as an introduction to the study of philosophy.

After the publication of my *Psychology,* I have repeatedly taken up inquiries throwing light upon psychological questions. In a paper entitled "Psychological Investigations" ("Psykologiske Undersögelser," 1889, later printed in *Vierteljahrschrift für wissenschaftliche Psychologie*) I tried especially to prove that there exists an immediate recognition (a simple perception), not to be explained through association of ideas, as no free conception plays any part in this process. It has interested me greatly to see that G. E. Müller arrived at the same result experimentally. What he terms *indefinite recognition* corresponds to my 'immediate' recognition. On the whole, I tried to prove that there are more forms of recognition (I found ten) than is usually assumed. From a physiological point of view we have in 'immediate' recognition a psychical expression for what the exercise of nerves and muscles may bring about. The paper also discusses several other questions, for instance, the various forms of associations of ideas and the Spinozistic view of 'the relation between soul and body.' The investigations contained in this paper led me to maintain that, since the usual psychological terms (words as well as phrases) have been created from observation of greatly complicated (differentiated) conditions, language is really lacking in exact expressions for elementary states of mind; accordingly, such conditions are often wrongly indicated by words and phrases which may at best be used as analogous terms. This may lead to misconceptions —and the explanation of 'simple recognition' by way of association is, in my opinion, an instance of this. In a paper entitled "The Psychological Basis for Logical Judgments" ("Det psykologiske Grundlag for logiske Domme," 1899, translated in the *Revue philosophique*) I undertook to describe an investigation which had to do partly with the relation between sense-perception, memory, and imagination, and partly with the relation between association of ideas and judgment. I consider judgment the outcome of analysis through which a transition takes place from one of the immediate perceptions (sensation, memory, imagination, and their intermediate forms) to the elements contained in them. When this transition has taken place, the point is to find that expression of the judgment which makes it best fitted to become part of a larger mental process. The expression of the judgment as absolute or partial identity proves the most satisfactory in this respect. In my central work, *Human Thought* (*Den menneskelige Tanke,* 1910, translated into German and French), I continued and concluded these investigations by establishing the forms of thought (categories) which, in the development of science, have been of the greatest importance, and the problems, based on these forms of thought, which belong to the very borderland of the scientific mode of thinking.

Since my work on *Human Thought* I have at various times returned to psychological studies, especially to the study of such subjects as had to be elucidated historically as well as scientifically. The first subject that presented itself to me was that of *Great Humor* (*Den store Humor,* 1916; the German translator has rendered the title by *Humor als Lebensgefühl*). The word *humor* has a long history, with which I have dealt to some extent in the book. It has not been my intention to treat of what I call 'little humor,' which is merely the ability to make people laugh. Humor, in my sense of the word, presupposes a serious foundation, which may be superiority, generosity, sadness, longing, understanding, or sympathy. Whereever a foundation of one of these qualities has been formed in the mind as a consequence of the process and labor of life, there is a possibility of a smile or of laughter in the face of the disharmonies, contradictions, disappointments, and sorrows of life. On account of the large scale which we cannot help using here, even the most trivial thing becomes significant. The humorist does not mock at others, nor at himself, but he may laugh at himself, just as he may laugh at others. Against the large background, held out by life in him and to him, it becomes possible for him to discover values even where pettiness or hostility seem to prevail. The relations between 'humor as a sense of life' and other modes of life are also gone into. I consider the chapter on tragedy and humor the most important one and look upon Socrates and Shakespeare as the greatest humorists. Unfortunately, I am no humorist myself, as I have plainly hinted towards the end of Chapter 6.

In my treatise on *Experience and Interpretation* (*Oplevelze og Tydning,* 1918, later translated into German), I compared two woman mystics, St. Theresa from the sixteenth century and Mlle. Cécile V. (pseudonym) from the twentieth century. Cécile's confessions have been published by her psychological father-confessor, Theodore Flournoy (*Arch. de psychol.,* May, 1915). The confessions of the two women (those of Theresa may be found in her *Life*, which was translated into French in 1670) offer interesting contributions to religious psychology. Ecstatic moments are known to both of them, but their attitudes towards them are different. Theresa considers them rather carefully (especially in her book on *The Castle of the Soul,* translated into French in 1670). She distinguishes between thoughts passing from subject to subject, from image to image, and such thoughts as concentrate on a single point, at one with our own inmost effort and desire. Although she distinguishes between experience and interpretation, the transition is very easily brought about by her; it simply consists in discovering biblical and ecclesiastical ideas in her experiences. For this she needs assistance from learned father-confessors (but she chooses them herself). On the other hand, the Protestant sister of her soul in *her* interpretation prefers images and symbols that come naturally to her. She even thought for a while to find a purer religion in the series of images which thus formed itself, than in the ideas of traditional religion. Finally, I remark that the distinction between experience and interpretation is of importance within all provinces of thought, even in pure science, as proved by the recent development of physics. It meant to me, personally, a corroboration of the view I had maintained some years previously in my *Philosophy of Religion.*

I also found use for psychological-historical methods in the paper on Pascal and Kierkegaard, which I was asked to contribute to the *Revue de Métaphysique et de Morale* in 1923 on the occasion of the third centenary celebration of Pascal's birth. As in the treatise just mentioned, a Catholic and a Protestant are here compared with one another. An uncompromising attitude towards the Church is common to both of them. Here, too, we have a psychological task of importance for the philosophy of religion. Great interest attached not only to the breaking away from the actual form of religion, which they were both facing, but also to the fact that, on their way towards this breach, they could not but throw light on the nature and conditions of the human mind. They have tried the strength of thought on the very borderline of thought, and they have also proved that, even where emotion and passion are running high, thoughts, like guiding stars, are still pointing the way of the spiritual movement. To illustrate the position of the two men within the realm of thought, I compare first their temperaments and characters, then their intellectual equipment, and, finally, their view of the problem of Christianity. As to the last point, the decisive problem to both of them is the contrast which they find between Protestantism and Catholicism, on one hand, and primitive Christianity, on the other; to the latter nothing was of any importance compared to the ecstatic expectation of the realm of God, while the Christianity of later ages absorbed elements both of classical culture and of the new civilization that developed independently of Christianity. Here there is a point of similarity between them which is of more than ordinary interest because it is a Protestant and a Catholic that are facing each other. Quite apart from the brilliant way in which they expose their attitudes, this is in itself of interest to all time. From their lonely points of vantage they have caught sight of great problems within religious psychology.

It is the task of experimental psychology to examine the conditions under which psychical phenomena are found, and perhaps numerically to determine grades and shades of such phenomena. But experimental and quantitative determination is only possible in the case of comparatively primitive psychical phenomena. Descriptive and analytical psychology ought, of course, to be collaborators. The way in which such investigations are to be used must, in the case of very concentrated and complicated phenomena, chiefly depend on qualitative observation and analysis, and one may be a good experimental psychologist without having that kind of ability; experimental psychology thus has no right—though it often does so—to appropriate the title of real or complete psychology.

There is a difficulty here about all psychological methods. We cannot be occupied by and interested in something and at the same moment investigate (through experiment, observation, or analysis) our own occupied and interested condition. In my *Ethics* and in my book on humor I have had opportunity to go into this question. The inability to investigate experience as it occurs does not preclude scientific analysis of the most interesting and important mental phenomena. For what cannot be done at the moment may be done later, when we have time and opportunity to compare all the outer and inner conditions of situations and actions. In my *Ethics* I had especial occasion to touch upon this point in connection with the question of what is usually called 'free will.' I express myself there to the effect that one cannot stand on one's head and legs at the same time, but one may perhaps first do the one and then the other. In all the provinces of science we have the interplay between synthesis and analysis, to which we do not always give proper heed. In ethics and aesthetics (not to speak of religious psychology) there is special reason to call attention to this fact. And modern physics has pointed out the necessity of such a 'complementary' mode of observation; indeed, physicists have even noticed an analogy here to the relation between 'volition and desire for causation' and thereby to the relation between experience and interpretation, description, and explanation.[1] Even in the philosophy of Spinoza we find at an important point a similar conception of the relation between the modes of thought to be used in various provinces.

When inquiring into the first principles of ethics, psychological understanding is a necessity. When the conception of causal explanation is defined sufficiently strictly, it becomes clear that ethical rules cannot be enforced in all cases, as is possible with logical, mathematical, and scientific rules. Psychological observation and historical experience show that human actions are judged from very different principles, corresponding to the psychological viewpoints of different beings, ages, and nations. Even though judgment from such different standpoints may go in the same direction, different reasons will often be given according to the special circumstances. Any ethical system that is to have theoretical and practical significance must be based on certain psychological and historical conditions, and by these the ethical type represented will be determined. I cannot here go into the question of the way in which I have myself tried to get over this difficulty, but must refer to the work on ethics which I published in 1887 and which has later been translated into several languages.

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[1] See my paper on the present status of the theory of cognition (*Dansk Videnskabernes Selskabs filosofiske Meddelelser,* 1930; in German in *Kant-stud.,* 1930).

I have also made use of psychological-historical methods in my attempt to outline a philosophy of religion. I was led to choose this line of approach by reading the *Confessions* of St. Augustine, where this great thinker and ardent churchman wants to account to himself for his embracing the Christian faith. The main motive proved to be the desire securely to retain what he had learned to value most in his life. Such a security he found only in the Christian dogmas. I tried then to see whether a similar line of thought could not be shown to exist in the other great representatives of positive religion, and whether it might not prove possible to unhold such a way of thinking outside any positive religion, values and ideals being asserted irrespective of changing times and psychological fluctuations. A standard for the evaluation of the special forms of religion may thus be obtained, the valuation being first and foremost determined by the attitude of human beings towards what is most valuable for them. And, under the struggle between various values, this standard will be the only possible one, the sublimity and potentiality of religious ideas being of decisive importance. The history of religion clearly shows that it is the union of these two characteristics that counts. The same standard must be made the basis of any evaluation of individual rules of life, as I have tried to show in a paper translated into French by Koyré as *Les Conceptions de la Vie.*

The interest that has never ceased to attract me to the study of the history and philosophy of religion has become to me more and more a purely psychological one. In this domain, where one may gain an insight into the inner life of profoundly religious personalities, I find such a fertility of emotion, such a grandeur of imagination, such a passion of the will as make it not only an attractive subject for the psychologist, but also give rise to the question as to whether forces like these can ever vanish from the mind of man without new psychical powers arising to form real substitutes for them.

My interest in psychology has not faded. My working day is now drawing near its end, but, if I might go on, I should be especially tempted by subjects which might be treated in a psychological-historical way. Still, the theory of cognition now holds the greatest fascination for me.

CHARLES H. JUDD

My parents were missionaries in northern India. They went to India shortly after the Sepoy Mutiny and served somewhat more than twenty years in various stations near Lucknow. During the last years of their stay both were in poor health. They returned to the United States with their family of three children—my two sisters and myself—in 1879. My father died during the following winter and my mother died in 1884, after an invalidism of four years.

The most vivid impressions of my childhood are related to the intense religious devotion of my parents. They were both evangelists and pietists. They had the most implicit faith in the literal interpretation of the Scriptures and exerted every endeavor to bring up their children in the same faith. I remember that I was disturbed in my high-school days by some books on evolution that came into my hands. I recognized the apparent conflict between the teachings which had been given in the family and the doctrines taught in the books. My curiosity was aroused, and I became greatly absorbed in books on anthropology and biology, a number of which I found in the public library.

The hope of my mother had been that I would enter the clergy, and when I started for college, I was disposed to prepare for the Methodist ministry, following my mother's wishes and the example of several generations of the family.

I have no doubt that my early intensive religious training had a large influence in directing my later interests toward a study of man and his mental life. I abandoned the plan of becoming a preacher early in my college career and have found my chief interest in scientific rather than religious solutions of the questions that arise with regard to man and his nature.

When my parents came to the United States after their long residence in India, I was six years of age. I had learned to read, as the youngest child of a family often does, through the unsystematic training given by the older members of the family. I spoke a combination of English and Hindustani which I found was a source of great amusement to my American relatives and to my playmates. I soon dropped the Hindustani and acquired the vernacular of my new environment.

I was entered very shortly after our arrival in America in the first grade of the public schools of Binghamton, New York. My father had selected Binghamton as a residence for two reasons. First, it was near his former home in Candor, New York, and, secondly, the schools of Binghamton were reported to him to be excellent. He was often quoted to me as saying that he could leave his children very little but hoped to launch them on their careers with a liberal education.

I went through the elementary schools and the high school of Binghamton, graduating from the latter in 1890. True to his expectation, my father had left his children little in the way of a material fortune. My older sister, with a great deal of effort, kept the family together, and my younger sister and I graduated together from the high school.

Three men stand out in my memory as exerting the largest influence over my education in Binghamton. The clergyman in the Methodist church which we attended took an interest in the orphan boy who lived across the street from the parsonage. The Rev. George Murray Colville did much—I hardly know how much—to arouse my intellectual interests. I recall one conversation of the many which I had with Dr. Colville. He called me into his study and remarked on the poor record that I was making in my high-school work. He said it was the judgment of my teachers that probably I had a very low grade of intellectual ability and he suggested that I show them that they were wrong. Certainly that conversation demonstrated the wisdom of Dr. Colville in dealing with a surly adolescent boy. I began to work with a vigor that astonished everybody, myself included.

Later, it was this same Dr. Colville who made it possible for me to go to a German university and obtain my doctor's degree. He lent me the money for the trip and generously waited long years for the return of his loan.

I had many books to read from Dr. Colville's library, and he took me on several trips, one to New York and one to Boston. I have no doubt that I owe to these trips and to my contacts with Dr. Colville much more than I can remember.

Another man who influenced me very greatly in my high-school days was the science teacher of the Binghampton High School, R. W. Griffiths. His domain was on the top floor of the high-school building. Here, in what was a kind of unfinished, spacious attic, one end had been partitioned off into a science laboratory. There were microscopes, chemical scales, physics apparatus, a three-inch telescope, and a heliostat, all closely packed together in this laboratory. Professor Griffiths presided over all the sciences. He used to allow a few of us who were especially interested to have keys to the laboratory, and mornings before school and afternoons after school we used the apparatus and added to the scientific enthusiasm which we gained from courses in Steele's *Physics* and other similar brief surveys of science. I recall that one day Professor Griffiths pointed out the difference between an expert and an amateur in their methods of handling a microscope. "Both of them seem to be careless," he said. "Both pick up the microscope in what seems to be the same easy-going way. But notice," he went on, "I pick it up by the base; you pick it up by the tube. You are ignorant."

Perhaps we did not learn much of the content of science from Professor Griffiths, but we acquired a lasting enthusiasm for Leyden jars and the things one sees in the field of a microscope and in the field of a three-inch telescope.

The third man who influenced my high-school years and, through them, my whole education was the principal of the high school, Eliot R. Payson, now professor at Rutgers College. Professor Payson taught us Latin and Greek and a great many things not written in the books. He was a teacher of unique qualities, as I learned afterwards when comparing my college preparation with that of my college classmates from other schools. Professor Payson was exacting, sometimes harsh, I should say. He was absolutely just, and he knew how to be familiar with boys without losing one grain of his dignity. He found a number of us one day engaged in disorder. He looked at us in silence for a few minutes while we stood petrified and then he calmly left us—wiser and, on the whole, I think, more moral beings. He gave us a very high grade of training in the classics. I have always thought better of his teaching than of the classics.

From high school I went in 1890 to Wesleyan University at Middletown, Connecticut. Wesleyan was, more than most colleges of that day, devoted to scientific research and literary production. On its staff were Professor Winchester in English literature, Professor Atwater in chemistry, Professor Rice in geology, Professor J. M. Van Vleck in mathematics, and a number of others whom I shall have occasion to mention as teachers who contributed directly to my special training.

The curriculum at Wesleyan in my day included a number of required courses. We were required to take analytical geometry in the sophomore year. It was my good fortune to be in a division taught by the physicist, Professor E. B. Rosa. Later I took several courses in physics under Professor Rosa. He was one of the clearest teachers I have ever had. I never had known what mathematics really meant until day after day he taught us analytical geometry. Later, in laboratory physics, too, he used to make everything so plain that science became more than a body of facts; it became a system of thinking.

In the junior year we were required among other subjects to study physiology. Here we were under Professor H. Conn, who came to Wesleyan after being trained at Johns Hopkins in those early days when graduate work was new in American universities. Afterwards I elected laboratory courses under Professor Conn and, because I was specializing in psychology, he let me work on nervous systems.

I was especially fortunate at that time to secure an introduction to Dr. F. K. Hallock of Cromwell. Dr. Hallock was associated at that time with his father in conducting a private sanitarium for neurasthenic patients. Dr. Hallock had studied in Vienna and, among other acquisitions which he had brought home with him, was a microtome large enough to make sections of a cat's whole brain. He had at the time I first met him imbedded in the well of this microtome the stem of a horse's brain and he proposed to me that I make sections and bring them up to his sanitarium, and we would look them over together. I thus came into the extraordinary fortune of a course in brain anatomy the like of which few undergraduates have ever taken. Professor Conn gave Dr. Hallock's microtome space in the biological laboratory, and I cut sections and once a week made a pilgrimage to Cromwell where I received the best kind of individual training in neurology.

The core and center of my training at Wesleyan I received from Professor A. C. Armstrong. Professor Armstrong had recently come from Princeton where he had been a pupil of President McCosh. He taught required logic to sophomores, required psychology to juniors, history of philosophy to juniors and seniors, and some electives, one of which was a seminar in James's psychology.

I do not now locate with any exactness the date when Professor Armstrong's teaching led me to decide that I would devote myself to the study of psychology. He has turned others also to the same study: Dearborn, Freeman, Thorndike, and others who, like myself, studied required psychology and later continued the study because of the interest which Professor Armstrong aroused in the subject.

I found myself in my junior year specializing in psychology. Professor Armstrong put into my hands Ladd's *Outlines of Physiological Psychology* and Sanford's *Experimental Psychology*. He encouraged me to make the study of nervous systems to which reference was made in an earlier paragraph, and he also encouraged me to take advanced laboratory courses in physics.

In my senior year I read James's two volumes in a seminar course with Professor Armstrong. In this class E. L. Thorndike also received his introduction to James. I took, in addition to the courses mentioned, Professor Armstrong's senior electives in the history of philosophy.

My associations with my first teacher of psychology have extended over so many years and have been so intimate that it is difficult for me to keep them in clear perspective. I remember that he gave me a great deal of individual attention during my undergraduate days. Wesleyan had no psychological laboratory at that time. Professor Armstrong, while protesting that he was no experimentalist, took me to the physics laboratory and tried out, with the help of apparatus that we borrowed there, some of Sanford's experiments. This statement will make clear what I mean when I say that Professor Armstrong was very generous in his care of a young undergraduate.

Later he brought me back to Wesleyan to teach in his department and he gave me the same kind of generous help while I was struggling with the problems that confront a young instructor.

Professor Armstrong taught me, too, the meaning of productivity. He had collected some interesting material on visualization, following the example of Galton's study. When I was a senior, he let me assist him in working up this material and he attached my name with his to an article which was published in the *Psychological Review*. This was my first attempt at scientific writing.

Professor Armstrong took me to the meeting of the American Psychological Association which was held at Columbia University in 1893. I saw the psychological laboratory, and more than that, I saw the galaxy of psychologists in attendance on the meeting. James was there and Dewey. Münsterberg was a newcomer and was heard on several occasions. Cattell and Farrand showed us their experiments. If I had been in any doubt as to my future calling,

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that meeting would, I think, have fixed my determination to become a psychologist.

Of the contributions to my mental life which Professor Armstrong made, there are two which I think are of such importance that they should be especially mentioned. In all of his teaching, in logic, psychology, and philosophy, he insisted that thinking should be systematic and coherent. He had a device which he frequently used in criticizing James. When James, the neurologist, wrote paragraphs which were highly flavored with materialism, Professor Armstrong would point out that James No. 3 was responsible for that passage. James No. 1 was the writer who believed that attention and the will actually exist as mental forces and as dominant factors in human nature. I do not remember how many Jameses there were, but I think there were four or five. The main point made was that a student who would be clear in his thinking must not be misled by the captivating enthusiasm of James's writing into accepting uncritically positions which are inconsistent.

The second important contribution of Professor Armstrong to my training was the truth that no thinking is complete which does not take into account in a broad way the contributions of all the leaders in the field. Professor Armstrong used Sully as the textbook in his required course in psychology, but he told us about Spencer and induced as many of us as possible to read Spencer. He told us, also, about Galton and Lloyd Morgan, about Bain and Wundt. He introduced us to James and Ladd. I remember very well that he repeatedly exhorted me to read extensively.

I took courses with Professor Armstrong during my last three years in college. To the clear and carefully formulated lectures which he gave us and to his methods of conducting discussions and recitations I owe more than to the example and instruction of any other teacher.

There are two episodes in my student life at Wesleyan outside of my regular courses that I think are worth mentioning. My sisters had moved to Middletown when I entered Wesleyan, and we lived near the campus. During the long summer vacations, when the college was closed, we had that part of the city almost to ourselves. One of these summers was especially quiet for me because my sisters went on a visit. I was allowed to have a key to the library by Mr. James, the college librarian, and, under an impulse which I suppose came from Professor Winchester's course in English literature, I read all day and long into the night Thackeray, Dickens, and Scott.

During the last summer vacation of my college career, I went to Worcester where one of my college acquaintances, John Bergstrom, was a student at Clark University. Bergstrom had offered to give me some help with German in preparation for my entrance the next year on a German university. He did more than that. He made me somewhat acquainted with the laboratory at Clark and he let me serve as subject in some of his experimental work.

On the social side I gained much at Wesleyan. My experience with the world was very limited, astonishingly limited when contrasted with the experience of the modern college youth. During the autumn of my senior year I served as manager of the football team. At that time Wesleyan was in what was known as the "big league" with Yale, Princeton, and Pennsylvania. I was injected into a group of associations which were wholly unfamiliar to me. I have no objective knowledge of the figure that I cut, but I am very well aware that I encountered situations that, to put it very mildly, severely taxed my social ingenuity.

In June, 1894, I graduated from Wesleyan and started for Leipzig to study in Wundt's laboratory. I have been asked many times by my own students, "What is the best course to follow in regard to study for the Doctor's degree? Should one go straight forward to the degree or should one teach for a few years and cultivate maturity and all that maturity implies?" My answer is not given without some reservations, but I always say that I am glad I took the risk that I did. To be sure, I was fortunate in the fact that I had a friend in Dr. Colville who lent me the money with which to make the expedition. If I had known in advance how many years of economy would be necessary to pay for my education, I am not sure that I would have had the boldness to start. Fortunately, I was so ignorant of financial matters that I went. My share of our patrimony had been more than used up during my college days and, when I started for Germany, it was an investment in advance of the only resource that I had—my profession.

On the steamer I fell in with a gracious gentleman who was taking a trip up the Rhine. He adopted me as a companion, and we had a short holiday together. I arrived in Leipzig early in July. I plunged at once into my first task which was to learn German. I went to Berlitz classes; I attended church regularly; I lived in a

German pension; I committed long pages of German to memory; I read a German grammar through every two weeks. In short, I lived with the German language. I remember the suspense with which I attended my first lecture in October. The summer's devotion to German had been successful; I could understand the lecture.

This is perhaps the point at which to introduce the statement that I never became adjusted to German life or to the Germans. My profound obligations to Wundt cannot be overstated, nor can my gain from study in German institutions, but the formality of the intercourse between German students was something against which my democratic training in America made me recoil most violently, and the snobbish attitude of most of the people whom I met seemed to me intolerable. I never became anything but a rank outsider and a very lonesome one at that.

There were some advantages in the strict limitations of my resources which I always had to recognize. I could do nothing but work. When the other Americans went on vacations, I stayed in Leipzig and read. The result was that in February, 1896, my thesis was accepted, and I was admitted to examination.

The Leipzig laboratory, or *Institut,* as it was called, was located in my day in the *Altes Trierisches Institut,* a dingy old building with little rooms arranged along a long corridor. The door was unlocked at 1:00 P.M. In the middle of the afternoon Wundt would arrive with the punctuality so characteristic of all his movements and would go down the long corridor to his office There would be a stir among the readers in the library and among the workers in the experimental rooms. At four o'clock, the *Institut* would empty into the neighboring lecture-room, and everybody would go to hear Wundt. Now and then Wundt would look into the experimental rooms, but, for the most part, he left the supervision of laboratory work to Meumann, who was *Privat Docent,* and to Kiesow, who was assistant in the laboratory.

Wundt's lectures were of special interest in those days because he was changing from the form recorded in the *Menschen und Thierseele* to the form which was later published in the *Grundriss der Psychologie.* Also there were rumors abroad of the *Völkerpsychologie,* though it was not good form to speak publicly about anything that Wundt was doing until the work was completed.

Meumann was dynamic in his lectures and was busy in his work on time perception. He conducted an *Einführungscursus* which met once a week and was designed to introduce all newcomers to the laboratory equipment and to the methods of experimental work.

Kiesow was engaged in his investigations of skin sensations. He was a great help to American students in general and to me in particular because he spoke English and also because he was willing to give advice on many matters.

There were other American students in the *Institut* beginning their work in the autumn of 1894. G. M. Stratton and Guy Tawney were the Americans with whom I was most intimately associated. It was a rule of the *Institut* that no student could begin an *Arbeit* until he had been a member of the *Institut* for a semester, We Americans, especially Tawney and I, circumvented this rule by beginning work on our investigations in our rooms. In the meantime, we could read in the library of the *Institut* and we could serve as *Versuchpersonen* in the various investigations that were going on in the *Institut.* Also, of course, we attended lectures.

The *Institut* was full of traditions of Americans and adopted Americans who had been there in earlier years: Hall, Cattell, Münsterberg, and, lately, Titchener. Münsterberg was recognized as quite heterodox; the others were regarded as more or less in good standing as Wundtians.

There was in the *Institut* very little respect for the leaders in American psychology who had received their training elsewhere than in Leipzig. Especially was there a very pronounced antipathy to James. James had done what was thought to be quite out of order; not only had he criticized Wundt but in some cases—as, for example, in discussing the *Innervationstheorie*—he had allowed his criticism to take the form of witty sarcasm. This was far too much. Not only so, but he had indulged in that remark about patient laboratory work in a land where they did not know what it means to be bored. As a result, diplomatic relations were promptly and totally suspended.

I had occasion to learn in a very pointed way the acuteness of the estrangement. I once asked why James was not translated into German along with most of the other books in the world. I found out in no uncertain terms that James was not regarded as a thinker of the first order.

In later years, after I had become in some measure an enthusiast for the teachings of Wundt, I prepared an article for the *Philosophical Review* on one aspect of the work of Wundt which is not commonly discussed in America, namely, his philosophical system. I had the temerity to think that my article might interest James, who, I found, thought of the Wundt school in somewhat the same way that the Leipzig *Institut* thought of the James school. I sent my article, which contained, I must admit, a great deal of interpretation as well as exposition, to James and besought him to find in it much in common with his own thinking. I received from James one of those postcard acknowledgments with which he used to favor his younger colleagues. The card contained the following seven words: "Would to God it were true. James."

The experimentation in the Leipzig laboratory during the two years that I was there was very largely devoted to problems of perception: space perception and time perception. The period of reaction-time experiments was past. Wundt's article on sensory and motor reaction-times had so far invalidated the older averages that there was an evident disposition to let the matter rest where Wundt's article had left it. There was some interest manifested in experiments on Weber's Law or, rather, in the general principle of relativity. In the main, perception was to the fore, and it was in this field that I prepared my thesis.

I was greatly impressed by the care with which Wundt read my thesis when it was handed to him. He did much of the editorial work for the *Psychologische Studien* and when he examined a thesis he also prepared it for publication. In due time I was summoned for a conference. Several changes were suggested in the text and finally we came to a point on which I had ventured an assertion without adequate evidence. Wundt asked for the evidence, and I offered as a defense of my position the remark that I thought what I had said was *a priori* probable. I received an answer that is so typical of what I was taught by Wundt throughout my contact with him that I quote it. *A priori ist garnichts wahrscheinlich.*

The spirit of empiricism which is exhibited in this remark was characteristic of all of Wundt's teaching. There are those who have accused Wundt of dogmatism. It has always seemed to me that those who make this charge overlook the enormous range of facts which Wundt had at his command. When he expressed a judgment there was back of it a wealth of material which has never been paralleled in any mind with which I have had contact.

Wundt was a prodigious worker. He followed a regular daily program and devoted all of his life to collecting materials for his ponderous tomes, to writing, to exmining theses and dealing with candidates for the degree.

I had a somewhat unusual opportunity to come into personal contact with him after I received my degree. His *Grundriss der Psychologie* had just come from the press. I made application to the publisher for permission to translate it into English. Wundt was known to be adverse to translations. Long years before an early edition of the *Grundzüge* had been translated into French with the unfortunate result that French references to Wundt's work were for the most part based on this early work. The publisher of the *Grundriss* secured Wundt's consent to the translation of his new book on condition that it was to be printed in Germany in small editions so that it could be revised whenever Wundt so directed. The publisher gave me permission to prepare the translation. Wundt insisted that he should see the proof. So it came about that on Thursday afternoons during the spring of 1896 I had conferences with Wundt lasting from fifteen minutes to half an hour. He made many suggestions and listened to such explanations as I had to offer of my translations of particular words and phrases. I brought him some American reviews of the *Grundriss* and listened with much interest to his comments.

At that time G. Stanley Hall was conducting in the *American Journal of Psychology* a section of short reviews. When Wundt read Hall's review of the *Grundriss,* he pointed out where, in his judgment, Hall had missed the chief contributions made by the book.

My two years in Leipzig were profitable not only because of my contacts within the *Institut* but also because of the opportunity which I had to take work in departments other than psychology. It was the custom of members of the *Institut* to take as one of the two minors required for the examination lectures in comparative anatomy with Leuckart. Leuckart was one of the most fluent and picturesque lecturers whom I have ever heard. He had a collection of specimens which were so striking that his lectures would have been illuminating if they had not been what they were in content and finished form. I had an experience in the first course which I took with him that made me aware of some of the shortcomings of the lecture method. I found at the end of the semester that my notes were nearly worthless. Everything seemed so obvious and easy to remember when Leuckart was talking that I had failed to take usable notes. This made it necessary for me to repeat the course, which I did with great profit.

My other minor was the history of pedagogy with Volkelt. I was not at that time especially interested in education. I was availing myself of the generally recognized privilege of taking one easy minor. I am fully convinced that the only reason that Volkelt passed me on the final examination was that he saw from the record that I had passed the two preceding examinations.

I took a course with Fleschig. He was, I am quite sure, one of the worst lecturers who ever gave a course to university students. He paid no attention whatsoever to his class and at times it was quite impossible to hear what he was saying to himself. He had, however, casts and slides which members of the class examined with profit after the lecturer had retired.

I found in Leipzig an uninterrupted opportunity to read. I have sometimes thought that students ought to be shut up in convents and kept from ready communication with their friends and relatives. It is not strictly true that I did nothing but read and work in the laboratory, but it was nearly so. The isolation of a student in a foreign land, especially a land which seems to him to be very foreign, will be understood only by one who has had an experience similar to that which I had in Leipzig. While isolation is emotionally depressing, it is intellectually stimulating in the sense that it keeps one hard at work so that one's stay may be short. I covered much ground in my reading and profited greatly by doing so. I read extensively in Wundt's writings. I became acquainted with Stumpf and Ebbinghaus. I read, also, some of the older works, especially those of Johannes Müller, of Ernst Heinrich Weber, and of Gustav Fechner.

My Leipzig training gave me a number of points of view which have been influential in all my later work. In the first place, I adopted without reservations Wundtian voluntarism. I have often thought that if Wundt had repeated in the later editions of his great work on physiological psychology the clear statement which he made earlier of his doctrine of innervation, he might have saved the world from the discussions of so-called "behaviorism" under which our science has been compelled to suffer in recent years. Wundt recognizes fully in all his writings and teachings the essentially active character of all mental life. He gives to the motor processes in the nervous organization of the individual a prominence which is beyond anything that the new behaviorists have ever been able to comprehend. The reason why some of the recent behaviorists flourish is that they have reduced psychology to a few simple formulas which can be carried in the mind without serious mental effort. They have simplified the mental universe as one might simplify the celestial world by mistaking the stars of the first and second magnitudes for the real cosmos.

Wundt never over-simplified. He recognized the complexity of mental life and he recognized the fact that mental life is a process and not a collection of items. His teaching was functional and synthetic, never atomistic and structural.

I accepted, also, and accept today, the doctrine of creative synthesis. Wundt never attempted to explain the higher mental processes by piling up great collections of lower mental processes. He believed in organic fusions. He believed that when mental processes become complex there appear new forms of experience which no lower stages of mental life exhibit or remotely resemble.

In the third place, I learned from Wundt what he calls the historical method of psychology. His contribution of a new method and a new body of material in his *Völkerpsychologie* does not seem to have exerted in America an influence at all comparable to the influence which was exerted by his earlier work in physiological and experimental psychology.

The explanation of American neglect of his monumental work seems to me to be that the physiological psychology preceded James and preceded the development of an independent American psychology. Wundtian social psychology came at a stage of the maturity of American psychology in which American workers were far less open to foreign impressions than they had been in the eighties and early nineties.

For my own part, I became absorbed in social psychology. It is my firm conviction that the great strides which the future will see in psychology will be not in the field of individual psychology but in the field of social psychology. The importance of language has appeared in every serious effort that has ever been made to study human nature. Wundt's two volumes on *Die Sprache* will, I believe, come to be thought of as his most important single contribution to psychology.

I have often commented on the fact that James seems to have had very little interest in social psychology and very little interest in the psychology of language. James evidently suffered in this respect from one of the blindnesses of human nature. American psychology has followed James rather than Wundt. In my judgment, this has been a grave misfortune and one that the future will have to remedy.

My personal interest in social psychology was intensified by a course of lectures which I attended under the historian Lamprecht. During the spring semester, after I had taken my examination and while I was translating the *Grundriss,* I took the opportunity to attend lectures in history. I learned, under Professor Marks, about the Franco-Prussian War and gained an impression of the attitude of Germans toward Bismarck. Lamprecht was lecturing on *Culturgeschichte des Deutschlands* and, as readers of his writings will readily understand, it was for me a course in social psychology.

No American who has taken a Doctor's examination in Germany is likely to forget the experience. The dress-suit worn in mid-afternoon, the high hat, and white gloves add to the excitement of the occasion. Wundt examined me first, and I remember with gratitude the first question which he asked me. He asked me in what part of the United States I lived. I have often thought that probably that was the only question I could have answered with assurance at that moment. I learned in the course of the ordeal that an examination is an opportunity to tell what one knows, not an effort to discover one's failings. I have tried in my later years as an examiner to emulate the example of those who conducted my examinations for the Doctor's degree.

In describing my Leipzig training, I must not fail to comment on the highly instructive experience which I had in translating the *Grundriss.* I secured all the recent good translations in psychology and philosophy that I could find and I used these as my guides. I made a complete translation of the *Grundriss* and when this first translation was completed, I destroyed it and did the work again. The work was profitable from many points of view. I gained much by studying Wundt's compact style. I learned an English psychological vocabulary which my sojourn in the midst of German experimentation had not given me. I became conscious of problems of expression that I had never thought of before. I may confess that some years later, when I revised the translation for a new edition, I found that I had been guilty of some glaring Germanisms in my first rendering.

During the summer of 1896 I came back to America and, with the opening of college in September, I began work in Professor Armstrong's department at Wesleyan as Instructor in Philosophy. My salary for the first year was $900, and I was glad to receive that much.

I taught the required sophomore course in logic to two divisions, an elective course in experimental psychology to advanced students who had completed Professor Armstrong's course in required psychology, and a course in the philosophical systems of Locke, Berkeley, and Hume.

Much of the work in the two advanced courses was conducted as lectures. I took the advice of some of my elders on the faculty and wrote out all my lectures. I left the carefully prepared lectures in my room, however, and in this way gained experience in the informal delivery of what I had to say. While this somewhat laborious preparation for my courses kept me very fully occupied, I am sure that I profited greatly by this type of literary drill.

I found among my colleagues on the faculty a number of men who, like myself, were receiving their first initiation into college teaching. We all lived in the dormitories and a number of us boarded together. The frank interchange of experiences between the members of this group contributed to my training quite as much as had the example of my teachers.

I remained at Wesleyan during two academic years. Encouraged by Professor Armstrong and by the spirit of scholarly productivity which pervaded the institution, I prepared a number of papers for the psychological journals and secured places on the programs of the American Psychological Association for the presentation of my materials. My chief interest was in problems of visual space perception.

In 1898 I accepted an appointment in the School of Pedagogy of New York University. This appointment gave me the title of professor and a salary which, though it was not large, made it possible to marry. I was married in my old home, Binghamton, New York, to Ella LeCompte.

I found on arriving at the School of Pedagogy that there was an aspect of the science of the human mind about which I knew nothing. I was probably as ill prepared to teach teachers as any young specialist in the theory of space perception and the history of psychology could be. I recall very well that I had on one occasion been lecturing enthusiastically on Weber's Law to a class of New York City teachers who were seeking increases in their salaries by listening to me, when I was interrupted by one of my gray-haired auditors with this question: "Professor, will you tell us how we can use this principle to improve our teaching of children?" I remember that question better than I do my answer. I made up my mind that I would have to begin to learn something about schools. I used my mornings for a number of months visiting schools.

I also tried, during my stay in New York, to enlarge the range of my experimentation. My first effort was to get some device by which I could make a record of pupils' language reactions. I used all the different kinds of phonograph diaphragms I could find and took records of voice vibrations on smoked paper. My experimentation convinced me that I was not securing records of the voice so much as records of the diaphragms, and I turned to some of the simpler motor processes. I secured some illuminating records of the motor processes involved in handwriting and some very interesting records of emotional reactions.

I conceived the idea, while carrying on this work, that a book should be written on the significance of motor processes, and I prepared a manuscript. In later years I condensed this manuscript into my paper on "Movement and Consciousness" in the *Yale Psychological Studies.* I submitted the book to two publishers and they declined it on the ground that it was too technical and too remote from public interest to justify its publication.

The experience of having a manuscript rejected is one which I am convinced every young writer should pass through. I learned that a manuscript must be prepared for readers, not for the satisfaction of the author. It is hardly fair to say that I learned this lesson fully at that time, but I certainly made some progress in the direction of a comprehension of that idea.

The last year of my stay in New York University was spent in intense and unremitting attention to university politics. It was the conviction of several of my colleagues and myself that something ought to be done in order to raise the standards in our division of the University. We expressed our views in season and out of season; we voted our views in the faculty; we appealed to the administration; and, by every means in our power, agitated reform. Our activities very naturally disturbed the administration, and on one morning early in May I found in my mail a polite request that I resign. I wrote a fervid document stating why I was resigning and found myself without a position and without any immediate prospects of securing one.

At this juncture one or two of my friends who were in a position to do so offered me openings in the commercial world. I made up my mind, however, that I was much less well fitted for commercial enterprises than I was for teaching and so I refused the offers and began a canvass of the academic institutions of the country. The University of Cincinnati took me in, first for the summer session and afterwards for the regular work of the academic year. I was certainly grateful for the shelter from economic desolation which the University of Cincinnati gave me.

I had also learned a valuable lesson about institutions. I found that New York University went on its way very much more easily than did I and my colleagues who had been bent on sudden reform. I have adopted since this first experience the methods of gradual reform whenever I have felt it necessary to change the practices of institutions with which I have been connected.

Before I went to Cincinnati I spent two months of arduous labor repairing a deficiency in my education which I had recognized as existing ever since I attempted to pass a minor examination in the history of education with Volkelt in Leipzig. I was to take a professorship in Cincinnati which had the pretentious title, Professor of Psychology and Pedagogy. Among the courses which I was expected to give was one in the history of education. So I read history of education all day for two months and learned much that was entirely new to me.

When I reached Cincinnati I found that the University had recently passed through a period of reorganization. The new faculty was very little welcome in Cincinnati society, and the internal organization of the institution was far from peaceful or settled. The president, who had been a biologist, believed in strict natural selection and had inaugurated a system of election of studies by students which was the most extreme in its freedom that has ever been tried anywhere in the world. The result was a degree of specialization on the part of some students and a scattering of energy on the part of others which I have never seen paralleled anywhere. At the beginning of each quarter there was a complete reshuffling of electives, guided by no one. In the midst of this chaos the faculty counted for nothing in the formation of policies. There were no faculty meetings except those which gathered now and then informally, privately, and always at some distance from the administrative offices.

I made a number of friends among the school people of the city and vicinity, during my brief stay of a year in Cincinnati, who have continued, to my great satisfaction, to be my friends ever since. I did not do much scientific work in Cincinnati because I was only a few paces ahead of my class in most of the work which I conducted. I was fully occupied attending educational meetings and teaching. I did some reading, but, for the most part, the year was scientifically a failure. I look upon it as a period of devotion to economic rescue work.

At the Christmas meeting of the American Psychological Association I met Professors Duncan and Sneath, and they invited me to come to Yale. I did not let them leave the room in which the invitation was issued before telling them that I would accept. It meant a considerable reduction in salary and a reduction in rank to leave Cincinnati for the position offered at Yale, but the compensation which I saw was in opportunity for scientific work.

At the end of the year at Cincinnati I moved to New Haven. My main stated duty was to teach three divisions of the introductory course in psychology. The course had been one of the most generally elected snaps in the curriculum. The students, quite unaware of any change in the administration of the course, had elected it with full confidence in tradition. Most of those who were interested in athletics and other absorbing student activities were registered in my course during the first year. The registration in the course was less by somewhat more than fifty per cent the second year.

Not only were my classes at first disappointing, but I found that the department to which I belonged was divided by serious internal discord, and, to make matters worse, the university administration was totally out of sympathy with much that was going on in the department. All this I learned when it was much too late to withdraw the enthusiastic acceptance which I had given to the invitation to become an instructor in psychology at Yale.

It took some years to correct the difficulties within and without the department. In the meantime, I found an opportunity for scientific work, the like of which I had never seen. The Yale laboratory was well equipped and above all was supplied with a workshop and a mechanic that made possible the construction of apparatus which in turn opened up limitless possibilities of experimentation.

A number of well-trained graduate students came to work in the laboratory, and experimentation went on at a gratifying pace. There were three lines of experimental work which received most of the attention of the group. First, and foremost, it was possible to greatly extend the studies in perception in which I had always been interested. A camera for kinetoscopic photographing of eye-movements was constructed and used for various lines of investigation. Secondly, a number of pieces of work were undertaken which dealt with motor processes. Thirdly, experiments in learning were launched. Several of these I carried on myself and found that they led me into the discussion of transfer of training which was at that time a very live topic.

In addition to experimentation in the laboratory, I found opportunity to give an advanced elective course in social psychology. From time to time I also gave courses in psychology for teachers.

I prepared and published a number of books during the seven years that I was at Yale. The first was entitled *Genetic Psychology for Teachers*. In this I attempted to show how learning to read is a process of social inheritance. I did the same for number and handwriting. I was able to combine with this discussion of the social aspects of education some results of experimentation. I became very keenly aware of the paucity of experimentally determined principles in education.

My second effort at book-making was in the field of general psychology. I prepared a general textbook for college classes and some laboratory manuals. These had a moderately wide use but never succeeded as have a number of the well-known American textbooks in general psychology.

The reports of laboratory work which were published in the *Monograph Supplements* of the *Psychological Review* were much more successful than the textbooks. In fact, I am of the opinion at this remote date that some of my early efforts to formulate systematic treatises in psychology were premature. In educational psychology, especially, there was not enough material at hand to justify a textbook.

In this connection I recall a remark made by James once when a group of us were watching the sun set behind the mountains in the Adirondacks. James had come for a short visit to the camp and was, as usual, the center of the group. He had recently published his *Talks to Teachers on Psychology*. In the course of the evening

I asked him what he thought of educational psychology. "Educational psychology," he answered, "I think there are about six weeks of it."

I am convinced that my own work would have been more profitable and probably more useful if I had devoted all of my time to experimentation. I often advise my younger colleagues to postpone the writing of textbooks and to produce as much first-hand experimental material as possible before they yield to the temptation to write general summaries.

Some day in the future I plan to gather up the work which has been done in various educational laboratories in a treatise on educational psychology. I shall interest myself in my old age in comparing this more mature work with the books I published in 1903 and 1907.

I was drawn into other lines of endeavor at Yale. There was some demand in the state for courses for teachers. Yale organized a summer school. The second year of this enterprise I was put in charge. The attendance was small and the experiment was discontinued. I had, however, become acquainted with the State Superintendent of Schools, Charles D. Hine, and through him I was brought into intimate relations with the school system of the state. I inspected high schools for the State Board of Education and participated in the state's program for the supervision of schools and the training of teachers. President Hadley favored my participation in this kind of work because it showed the state that the officers of Yale were ready to render public service whenever possible.

In 1907, I gained my promotion to a full professorship and was made Director of the Psychological Laboratory. I found that I had unlimited freedom for scientific work and that I was in command of material equipment which was superior to anything I had known anywhere else. The only limitation from which the position suffered was that the number of graduate students was small. Yale was essentially a college. Graduate work at that date was far outdistanced by undergraduate. When the invitation to move to Chicago came, there were two considerations which led me to accept, namely, the prospects of more graduate students and a greater enthusiasm, as I thought, for the extension of investigation into practical fields.

Before I enter upon a discussion of my experiences in my present position, it may be proper to state some of the scientific views which my Yale experience had tended to fix in my mind. With regard to a number of these scientific views, I am sure that it must be said I brought them to Yale from Wesleyan or Leipzig, but they certainly gained in maturity during the seven years at New Haven.

I was convinced by the results of my experimental work in the Yale laboratory that the higher mental processes are not of the same pattern as lower mental processes. I had observed the growing tendency to seek the explanation of human mental life through experiments with animals, and I had become convinced that this effort to explain the complex phenomena of mental life as mere summations of the elements that appear in lower forms of behavior is fundamentally wrong.

In my presidential address before the American Psychological Association in 1909 I discussed at length the view which I still hold that evolution has produced in human life a group of unique complex facts which cannot be adequately explained by resolving them into their elements. Human mental life is a unique product of organization. Through evolution certain complexes have been produced which are new and potent causes in the world; among these is human consciousness.

I have sometimes regretted that my duties in other lines have kept me from further vigorous participation in some of the recent discussions that have been going on in psychology. I find myself so much in harmony with the conclusions of my colleague, C. Judson Herrick, in those chapters in his volume on *Brains of Rats and Men* in which he attacks the non-introspectionists that I am impatient with the slow assimilation into current psychological literature of his cogent arguments. I am also encouraged by the recent paper by Shepherd Ivory Franz on *The Evolution of an Idea: How the Brain Works.* These writers seem to me to reinforce very powerfully the conclusion that issued from all the earlier experimental work, namely, the conclusion that all the higher neural and mental processes are the products of organization.

The experimental work which I did in the Yale laboratory led me, also, into the field of learning. I became very much interested in an examination of the processes by which training transfers. My experiments made it perfectly clear that wherever conditions are favorable to generalization there is transfer. The nature of generalization is such that no simple formula like that of the presence of identical elements is remotely adequate. Generalization is a type of organized mental reaction; it depends on creative synthesis.

Another general principle which was to my mind fully established by the experiments in the Yale laboratory is discussed in my paper entitled "Movement and Consciousness" published in the *Monograph Supplement of the Psychological Review,* No. 29.

I reviewed in that paper the views of a number of writers, especially those of Dewey and Münsterberg, and attempted to show that, while the content of mental life is derived from impressions, the forms into which these impressions are organized are conditioned by the motor processes.

I am convinced that the theory which was originally formulated and called the innervation theory was an important anticipation of the contribution which James made to psychology in his statement that emotions and certain other aspects of mental life are conditioned by bodily movements rather than by sensory impressions.

I look forward to the time when a true behavioristic theory will be accepted in psychology rather than the pseudo-behaviorism, which is nothing but non-introspectionalism, that has been the boast of some members of the present generation of popular writers on psychology. There is much productive laboratory work to be done on human behavior, especially the higher forms of behavior, such as language. When this experimental work is done there will be very little ground for the shallow dogmatism of the self-styled behaviorists.

In June, 1909, I left Yale to enter upon the duties of the post which I now occupy. I came to Chicago as the administrative officer in charge of the School of Education of the University of Chicago. My personal opportunities for research have been much less than they were at Yale. On the other hand, it has been possible, with a much larger staff and with the cooperation of a large body of graduate students, to carry on investigations on a scale incomparably larger than would have been possible anywhere else.

During the twenty-two years that I have been in Chicago I have been able to carry on four lines of scientific work in the special field of educational psychology. First, I have shared in the analysis of reading to which the Department of Education has given much attention. Secondly, I made an effort in 1915 to formulate and discuss the major psychological problems which arise in high-school education. Thirdly, I have done some experimental work dealing with number consciousness. Fourthly, I have taken the first step in the direction of formulating the social psychology on the basis of which I believe all sound education must ultimately rest.

Before I attempt to outline somewhat more fully the results of these four lines of work, I pause to comment on the state of general psychology as I see it from the point of view of my specialty. It seems to me that psychology suffers from a lack of fundamental unity. Many of those who started out, as I did, with interest in the general science of psychology have become, as I must confess I am myself, engrossed in a special branch or phase of the science. The result is that very few workers are left in psychology who have escaped the dangers of narrow specialization.

There are, for example, the investigators of the neural and mental reactions of animals. It is hardly to be expected that these specialists will be concerned except in a remote way with the psychology of the higher mental processes. There are those who deal almost exclusively in tests and in the statistical manipulation of the results of tests. The findings of this group are so spectacular that it can hardly be expected that its members will follow the road of long, arduous labor in the laboratory. The clinical psychologist has had such éclat during recent years that he is justified in his own eyes in going his way without giving attention to normal psychology. Educational psychology has been in such demand that its devotees have often thought of their branch of the science as the only branch worth reading about.

I believe the disintegration of psychology will bring with it serious consequences. One such consequence which it seems to me is already evident is that there is much hasty and speculative generalization. The animal psychologist holds that all mental life can be explained in terms of his special findings. The maker of tests speculates without any inhibitions on the nature of intelligence. The educational psychologist is quite competent, in his own mind, to discuss all the social institutions of modern times and to guide conduct, private and public.

Another consequence of the decentralized state of psychology appears in the fact that students in other fields, finding no adequate psychological principles to guide their thinking, begin to construct their own psychology. This they do in many cases without utilizing the methods of analysis and investigation that psychology has already devised. The most striking example of this is to be seen in the fact that much of the social psychology is being written by sociologists or by writers who are quite willing to speculate rather than develop a body of verifiable principles.

Psychology is paying the price of its popularity and of its intense human appeal. It will, it seems to me, have to undertake in the near future much fundamental research. It will have to be cured of the idea that a hundred loosely related facts can be welded into a body of scientific truth by averaging discordant tendencies and covering up most of the facts through statistical juggling. It is my belief that all of us who are at work in special fields suffer from the lack of well-founded general doctrines.

The general observations made in the last few paragraphs will explain the reason why I have attempted in my own work to hold to experimental and analytical methods. In the investigations which have been made in reading by our department, we have found it highly profitable to make detailed studies of individual performances and of the reactions of individuals to a great variety of special conditions. We have used reading tests, but we have thought of the results of these tests merely as starting-points. We believe that it is inherent in the nature of a test to reveal a present condition rather than to uncover a fundamental cause. A test may show, for example, that a certain individual is a poor reader, but the test does not tell what is the cause of the deficiency. The underlying cause of the present condition can be discovered only by painstaking analysis.

One of the methods which has proved highly productive in the analysis of reading processes is the method of photographing the eyes of a subject while he is reading. The apparatus which has been used by a number of members of our department was originally designed by W. F. Dearborn when he was on the faculty of the School of Education. The apparatus has been enlarged and reconstructed by a number of users. The films which are secured by means of this apparatus can be translated only with the most arduous labor. It requires from three to five hours to decipher an ordinary record. I mention this fact in order to make clear what I believe to be one of the strongest reasons for the infrequency with which graduate students select thesis subjects requiring the type of experimental technique which we have found to be so highly productive.

Difficult as the technique is, it has produced results of far-reaching importance. Huey called attention in 1908 to the difference between oral and silent reading. It was not, however, until that difference had been established by numerous photographic records that teachers accepted the distinction and began to modify their teaching so as to give special training in silent reading.

Investigations later than those which made clear the difference between oral and silent reading have shown that the reading of mature individuals is of several different patterns according as conditions of reading change. Reading undertaken for the purpose of preparing to answer questions is very different from reading for one's own satisfaction. Reading of an arithmetic problem is different in character from reading of ordinary prose.

I have pointed out in some detail the results which have been secured by the application of laboratory methods to the study of one educational problem in order to show that there is a very promising field for future work open to anyone who is willing to adopt experimental methods in the study of mental processes.

In 1915, I prepared a psychology of high-school subjects. There was very little empirical material at hand on which to base such a work. In 1927, I completely rewrote the book and found it possible to incorporate much new material of an empirical type.

The psychology of high-school subjects is to my mind one of the most promising fields for further work. The opportunities to which this statement refers may perhaps be described more adequately by the general statement that very little is definitely known regarding the character and genesis of any of the higher mental processes. We know much about space perception, but we are unable to give any adequate account of what goes on in the mind of a student who is trying to follow the reasoning of a geometry textbook. The higher mental processes are, of course, the processes which it is most important to call forth in the mind of a pupil. At the present time we are ignorant regarding methods of teaching in higher institutions very largely because we are in the dark about the nature of abstract thought and logical reasoning.

I hope to find time and opportunity in the future to follow the line of inquiry which the preceding remark has indicated as promising. In the meantime, there is one subject of instruction in the elementary school which has long seemed to me to call for attention; that is arithmetic. McLellan and Dewey wrote on the psychology of arithmetic in 1895, but, like many of the early writers in educational psychology, they speculated rather than reported experiments. While I was at Yale, I began some experiments on number which were interrupted by the move to Chicago. In the years immediately preceding 1926, I found opportunity to continue these experiments, and in 1926 I prepared and published a monograph entitled *Psychological Analysis of the Fundamentals of Arithmetic.* I am gratified to be able to add that one of my colleagues, Professor Buswell, and one of the former students of our department, Professor Brownell, have undertaken experimental work in arithmetic and have already gone far enough materially to amplify my results.

The urgency of the problem which confronts the schools in their teaching of arithmetic can be made clear by pointing out the fact that there are more failures in arithmetic in the intermediate grades than in any other subject. Apparently there has been no adequate understanding on the part of teachers of the ends at which they should aim or of the stages through which the pupil passes in his efforts to master arithmetic.

The fact that psychology has been slow in coming to the rescue of the schools in this matter is to be accounted for in part at least by the difficulty of devising any laboratory methods of analyzing number ideas. Most of a pupil's use of number is so subjective that it is difficult to secure records by means of which analysis can be made. In this respect, number consciousness is very much like reasoning and all the higher forms of mental activity. It has been possible, however, to devise some very productive methods of investigation in spite of the subjective character of these processes, and I believe that it will be possible in the future to complete the study in this and the other fields to which reference has been made.

The last constructive piece of work which I have been able to complete is in a field in which I am more interested than in any other. In 1926, I published a book for which I had been collecting the material ever since my Leipzig days. It is entitled *Psychology of Social Institutions.*

The chief purpose of this book is to lay the foundation for a systematic treatise on educational psychology which I hope to prepare. The thesis which I defend throughout the book is that collective intellectual effort has brought into existence certain products, such as tools, number, language, and government, which products could never have been created by the individual. These products of cooperative effort are effective causes of new mental activities, which they initiate and condition. Thus when language is once produced, it makes possible a type of thought activity which could never have appeared before there was a language.

The profound significance for all the social sciences and for educational systems of a valid explanation of social institutions is, I believe, gradually making itself felt. The inadequacy of individual psychology as a basis for the scientific understanding of society has been pointed out vigorously by both psychologists and representatives of the special social sciences. Certainly, many of the modern conceptions of education are outgrowths of an individualistic philosophy which has no justification in the experience of the race.

In this account of the psychological investigations and writings which I have been able to complete at Chicago, no reference has been made to the other lines of work which I have carried on. I have had certain administrative responsibilities and I have usually taught a full program, which, in the University of Chicago, amounts to eight hours per week. My administrative relations have brought me into contact with educational associations and various national committees not engaged in the discussion of psychological problems.

It is difficult to say how far these non-psychological activities have had positive influence on my psychological thinking. It is easily possible to show that, negatively, they have greatly limited the time and energy which I could devote to laboratory work.

It is my judgment that there is a very intimate relation between my practical undertakings and my scientific interests. I am satisfied that far more genuine research in educational psychology has been completed by my associates, for whom I have been able to arrange working conditions, than could have been accomplished by my individual efforts. So far as my personal work is concerned, I am disposed to believe that it has probably been about as extensive as it would have been under any conditions. Research seems to me to be a periodic affair, not a continuous process. I find that now and then I can concentrate on a problem and secure results. Between research periods there are periods when teaching helps to formulate one's ideas and when contacts which seem somewhat foreign to research lead to the acquisition of wholly new and very productive ideas.

Administratively, I favor for persons of my temperament at least a program of varied activities. It is my observation that most of my colleagues, as well as myself, need periods of release from each kind of duty which they perform in order that they may come back to this duty with new points of view and with new enthusiasm. In arranging work for the staff with which I am associated, I have tried to provide men with time for research when they reach the point where they can profitably devote all their energy to some problem which is well advanced toward solution. It is my belief that it is wasteful administrative policy to subsidize for research men who do not begin work until they are released from other duties.

Furthermore, I have no sympathy whatsoever with those people who complain that they are not given opportunity for research. I believe that the world has a place for any man who will demonstrate that he can do research work. The demonstration is, however, the first step.

I have sometimes been upbraided by my friends in psychology for not continuing the kind of work which I was doing at Yale. Certainly I believe in fundamental research and I am sure that institutions should make such fundamental research possible by creating attractive positions for men. I am equally clear in my belief that fundamental research in psychology can be carried on best by workers who are concerned, as I am, with the applications of such research to human affairs. I am disposed to think that much depends on an individual's temperament when he is choosing a field of scientific work. For one who is interested in social psychology, it seems to me quite unthinkable that there should be a lack of interest in the educational and social movements of the present day. It may be that there is a phase or branch of fundamental psychological research which is legitimately distinguished from what I have called social psychology. Personally, I think social phenomena are so much more illuminating as manifestations of what active human intelligence tends to do than are any purely individual exhibitions of intelligence that I cannot believe that individual psychology when detached from social psychology is a fundamental science.

The editors of this series of biographical sketches have suggested that some comments be included as to the lines of work which may properly be recommended to students who are beginning their work. I feel sure that such recommendations as I have to make are clear from my account of my own experience. I should say to students: Select teachers who have broad interests; read extensively; become interested in analytical methods; be sure to include laboratory work as an important part of training; and, above all, add to analysis and laboratory methods, the methods of study which Wundt has called historical, the methods of social psychology.

As to the most promising fields for research, I believe they are to be found in the intensive study of the higher mental processes. I believe that the distinguishing fact in human life is a form of intelligence which is indeed the product of evolution from below but is so much more complex than any forms of habit or animal intelligence that categories totally different from those of biology must be developed in science for the classification and understanding of human behaviors. If psychology is to assume its proper function as the fundamental social science, it must, I believe, discover and describe clearly the nature and causal effectiveness of the higher forms of mental activity.

C. LLOYD MORGAN

I

How it came about that a kindly friend, the rector of our parish, thought it worth while to bid a school-boy in his 'teens to tackle Berkeley's *Principles* and earlier *Dialogues* was on this wise. I may have mentioned an incident at my grandfather's table, years before, when he gravely asserted that if the housekeeper, the cook, the errand boy, the shopman, and their maker, had not thought of sausages there would be no sausages for breakfast; and that if none of us saw them, smelled them, or tasted them, we should have no idea of there being such things as sausages. He then plied poor little me with puzzling questions, to my great discomfort. "I like to make the youngsters think," he would say on such occasions. And the autobiographical point is that he *did* make a youngster think.

Anyhow, in some such way as this, the good rector and I slid on to the topic. He, too, plied me with questions, sympathetically amused, no doubt, by my rather callow interest, very shallow knowledge, and quite confident, common-sense, Johnsonian attitude. Then he said: "Why not read Berkeley at first hand, just as you read Wordsworth or Shelley" (of whom we had been talking)? "Might it not be well to do so before expressing a third-hand opinion based on someone's second-hand rendering which, after all, shows only *his* reaction to the problem under discussion? Drink always at the fountain-head in matters in which you are really interested."

I did so; and this was my first-hand introduction to philosophy. I can date it as near the close of my school-days because I can picture myself sitting in the hedge-shade in a meadow near a bend of a familiar stream. Never mind rod and line and possible perch. Here was Berkeley (in blue-grey boards) teaching me to fish for ideas in the deeper waters of the mind.

And now, after more than sixty years, I ask: What was then *my* reaction to his teaching? I find it hard to say. My then-reaction is so colored by now-reaction that I cannot be sure what it then was.

Herein lies a difficulty in any autobiographical sketch which purports to deal with one's mental development. It is a story of oneself in the past, read in the light of one's present self. There is much supplementary inference—often erroneous inference—wherein "must have been" masquerades as 'was so.' The story is pretty sure to be too neat and trim.

When my school-days were over and I entered on a course of training in science, my kind monitor still urged me to keep this earlier interest alive, and sketched out a program of selected, but first-hand, reading. This led me back through Locke to Descartes, then onwards to Hume, and thence to some acquaintance with common-sense Reid. Spinoza and Leibnitz were subsequently intercalated. Kant was reserved for later on. Then go back to Plato and Aristotle. This was the program. And this, such as it was, constituted my avenue of approach to philosophical thought wholly outside the stimulating give-and-take of lectures and classroom discussion.

I need say little of my antecedent school-days—just enough to show the direction of the educational wind-currents. My father, a Wykehamist, had sent my elder brother to Winchester; but, for reasons which form no part of this story, my lot was different. I was sent to a grammar school, not yet modernized but conducted on Winchester tradition by a Wykehamist as headmaster. Here Latin and Greek reigned supreme, with some mathematics, a little English history, and a very little (rather apologetic) French thrown in. I worked my way up through the school, and took third place on the list in the last examination.

To my modest introduction to the Classics under one who was accounted by my betters as a scholar, I owe much. My rather bored attitude in the lower school was insensibly transformed when I reached sixth-form status. I was led to feel that charm which supersedes drudgery and lies at the root of the matter.

For the rest, I stole time to read more English poetry than most boys—ranging from Shelley, Keats, and Wordsworth, to Tommy Moore and the Ingoldsby Legends—with a background of wonder as to how it is done; and with vain attempts to do likewise in juvenile verse. However poor and inadequate the outcome, an attempt myself 'to do likewise' is, I think, part of my mental make-up.

About a year before I left school the headmaster called me into his study after prayers. *The Idylls of the King* lay on the table. "Have you read this?" "Yes, sir." "Then read aloud to me one or two passages—where you like." I did as I was told. "I thought so," he said, "from the way you recited that ode of Horace this morning. But let yourself go and don't be afraid of showing me how you like it. Be careful to give every syllable its full value, *and* give voice to the rhythm of thought, not only of words. You missed fire here and there; for example . . .," and he read me one of the passages so as to bring out meaning that had escaped me. "Well, my boy, would you care to repeat the dose?" Once a week during my last year the dose was repeated with widening range. And each Sunday evening he read me Keble's hymn from the *Christian Year*.

Such was the master; such the boy.

Apart from this inspiring mark of favor, more welcome than half a dozen prizes, what I have said of my school-days is so commonplace a sample of boy-life in hundreds of old-fashioned grammar schools in the sixties of last century that it is scarcely worthy of record, save insofar as it goes to show that in school and out of school my early education was humanistic and literary.

I took my full share in games and athletics, and was a bit of a boy-naturalist, collecting most things, but with an itch to get at the "go" of them outside the cabinet shelf. Under the influence of an uncle, I was an observer of birds and learned something of their song-notes, plumage, and manner of life.

II

My school-days over, what next? To follow my brother to Oxford was ruled out of court. How about mining engineering as a profession? My father, a lawyer, was concerned in two or three mining companies and in touch with engineers. Why not go to the Royal School of Mines in London, as they advised? I was doubtful whether I had any bent that way. I really did not know what I wanted to do, or what good I was for anything. But it seemed better than an office stool as an alternative.

A trivial incident is here in place. It goes back to my school-days. I had laboriously trudged up the traditional pathway to mathematics. One Easter holiday an old friend of the family, a pretty high wrangler, asked me how I was getting on. "You have begun trigonometry, I hear. Would you like me to give you a little help?" Politely I replied: "Thank you very much," but with little gratitude at heart. "Come along then." He took me into his den. "This is a Gunter's quadrant. We may as well begin by measuring the height of the Araucaria on your father's lawn. Then with this surveyor's compass, we'll make a scale plan of the garden"—a large one and nowise formal. I gasped. 'Trig' had been for me so irretrievably booky and blackboardy.

In a few days I was as keen as mustard—much to my friend's satisfaction. He had tapped the practical vein in me. "My dear boy," he said, "you'll never learn the good of anything—Gunter's quadrant or mathematics—till you use it—till you do something with it."

So when he was consulted with regard to my embarking on science as an avenue to a profession, he said: "By all means. He'll take to it as a duck takes to water." And, accordingly, in 1869 I entered on a full course leading up to the diploma or associateship in mining and metallurgy. Of this I need say little. I worked pretty hard and with zest; took such medals and a scholarship as lay *en route;* and was reckoned senior student at the finish.

But the scientific method, rather than its prospective application in a professional career, was what intrigued me. The practical vein was still there. Do something with it—yes. But the top men in science used it for the advance of knowledge for its own sake.

Meanwhile, I kept close, though necessarily occasional, touch with Berkeley and Co., wondering how best I could square this new world of physical science with that old realm of philosophy, or whether I must choose between them, or perhaps keep them in separate mental compartments. Anyhow, the good old folk must be brought up to date in respect of their physical outlook. I made time to read all I could lay hands on that had bearing on the matter.

Naturally, I turned to Huxley, among others. In his *Lay Sermons* (1870), for example, there was much grist to my mill. The discourse on the "Physical Basis of Life" gave pause. Here was science, on the one hand, dealing with the how of events and, on the other hand, notions like 'aquosity' and 'vitality' posing as Causes of these events. Pressed home, was this distinction valid? Was all reference to Cause (in some sense) to be banned from the council-chamber of science? Then there was the Discourse on Descartes' "Method." In *Macmillan's Magazine* (1871) there was "Bishop Berkeley on the Metaphysics of Sensation." So much turned on physiology as an offshoot of physical science; and of this I knew nothing save at second-hand through reading. Just how did a science of mind link up with this science of the physical basis of mind? Mind, as such, seemed somehow to count in the course of human affairs. Did Huxley make clear to me the manner in which it does count? I was puzzled.

It so chanced that I was called on to take the chair at the second of a series of annual dinners for students and staff. I was honored by the support of Huxley on my right. He courteously sounded me on my aims and prospects. I then thanked him for what he had done for me and for others, touched on my interest in Berkeley, and lamented my ignorance of biology. In the intervals between speeches he returned to the topic; gave me of his riches without emphasizing my poverty. And, as he bade me a kindly 'Good-night,' suggested that, if it was possible, I might as well put in a year under him.

Shortly after this I had the opportunity of a three-months' trip to America, in the capacity of traveling tutor to a member of a wealthy family whom I was to meet in Chicago. I could take my young charge where I liked 'to see a bit of the world.' We cut across to Cairo on the Mississippi; went down the river to New Orleans; worked up to Washington through the Alleghanies; took leisurely ship from New York to Rio, calling at several of the West Indies, at Para, Bahia, and Pernambuco; spent a fortnight in Brazil, and thence returned to England.

It was an eye-opener. Apart from much else, the naturalist within me was stirred, with the *Naturalist's Voyage* at my elbow. And not only this. Here was the leisure seriously to tackle the *Origin of Species* and the *Descent of Man*. Yes; I must follow Huxley's advice when I got home if my good father would consent to keeping me on hand for another year or so. I knew something of physical science with my nose up against the facts which lay open to observation. I knew a little philosophy, though limited in range. I was so far a psychologist as to be constantly at work in the laboratory of my own mind. A plank in my platform was lacking—some first-hand acquaintance with comparative anatomy and physiology.

So, on my return, I put in my year under the master with scarce other motive than to add this new plank to my platform of outlook on science and philosophy. Whatever else I might do or leave undone (after all this oddly arranged and academically unconventional preparation), it must still be my chief aim to broaden and deepen this outlook.

III

I am, as may already be obvious enough, unskilled in writing an autobiography of mental development. It is itself an exercise in psychology; but it here purports to show how the psychological attitude, as such, comes into my picture, and the bearing of 'other considerations' on that attitude.

When something stands out clearly in reminiscence, it calls, I take it, for autobiographical mention. It stands out clearly that, at the close of one of his lectures on physics (1870), Frederick Guthrie, in response to some question, gave me an offprint of W. K. Clifford's Royal Institution Discourse on "Theories of the Physical Forces." It called forth a strong reaction on my part.

"There are to be considered," he says near the outset, "two different answers to the question, 'What lies at the bottom of things?' The two answers correspond to two different ways of stating the question; namely, first, 'Why do things happen?' and, secondly, 'What is it precisely that does happen?' " He contended "that the first question is external to the province of science; but that the second is exactly the question to which science is always trying to find the answer." This was in line with Huxley's dismissal of 'aquosity' and 'vitality.'

Was it sound policy from the point of view of method? Could it consistently be carried out? If so, should it not be applicable in psychology no less than in physics? Was it so applied in current discussion of mental procedure? The policy had bearing on psychological interpretation.

In his lecture, Guthrie had said something about the different senses in which the word 'continuity' might be used, and referred to Clifford's illustration from the 'wheel of life,' the toy predecessor of the cinema show. Here what interested me was the interplay between the orderly 'jumps' in the zoetrope and the pretty smooth movement that I saw. No doubt there was some flicker. But might not that be because the wheel of life was too coarse-grained in its jumpiness? When a disc with equal black and white sectors is more and more rapidly rotated there is a stage at which the jumps are so fine-grained that flicker quite disappears. And the light grey I then see may be matched with a pale grey on a still piece of paper. But is there jumpiness in this latter case? And, by simple inspection, can I tell whether I am looking through a tube at an isolated patch of a rotating disc or a patch of quiescent light grey paper? Can I say whether, at physical source, there is jumpiness or not? And so on. It seemed to me that events in the physical world might be jumpy or smooth-running; and that, on the basis of a simple inspection, it is hard to say which, so long as the jumps are fine-grained and orderly. But I could proceed on the hypothesis that they are all jumpy, or that they are all smooth-running, or that some are continuous (in this sense) and some discontinuous; and see how matters worked out. It seemed to me, however, that, on the jumpy hypothesis, my direct experience of coarse-grained jumps was itself jumpy; of very fine-grained jumps, smooth-running.

Turning, then, to the mental side of the account, Clifford told me that he had no doubt whatever that "the wheel of life is really an illustration and type of every moment of our existence." Did 'existence' here mean 'experience'? If so, all our apparently smooth-running experience is really jumpy, but so fine-grained as to be reckoned as smooth. Was this because the retina has discrete, and in that sense atomic, nerve-endings in the mosaic of rods and cones? Was the movement of the eyes in following a smooth-running billiard ball itself not smooth but step-like? If so, this was anatomical and physiological. Was it also mental as given in our conscious experience? I did not know and was puzzled.

Clifford then passed to the hypothesis of smooth continuity in the physical world. Here, though parts of the argument were beyond me, matters, on the whole, seemed pretty plain sailing. But I still in a more or less fumbling way wondered whether it showed that *all* physical events were so comprised. Was the passage from one physical 'state' to another, or from 'these' physical 'properties' to 'those,' fundamentally smooth—a slide and not a step? The difference in structure and properties between chemical compounds and mechanical mixtures had been dinned into us. And it seemed to me that, taking them at their face-value, chemical events were relatively jumpy, whereas mechanical events were smooth-running, though both conform to a natural order. Clifford's argument applied to the latter. Did it apply to the former? If not, must we not reckon with physical events (in the broad sense of the word 'physical'), some of which are jumpy and some smooth-running? I was only a beginner and I could not answer this question. But I could ask it and meanwhile remain puzzled.

Let me repeat that I may be reading into the 'then' more definite puzzledom than there then was. And, no doubt, I here state it in terms more clear-cut than I should then have used. But, in however ill-defined a form, the kind of questions I have set down were then itching for answers. And I can now picture in imagery just where I sat or how I sought council from my 'Gunter's quadrant' friend, when I then tried to worry things out.

But is all this worth saying? It is for others to judge. Save under gentle editorial pressure, I should not be saying anything autobiographical. What then does it show? It shows that, if not in the foreground, still lurking in the background, was the question—the old Berkeley question—where and how does the mind come in? Thus far my bias was psychological. But thus far it was little more than a bias. It and the like show, too, thus early, some thirst for knowledge in itself and not only to the end of technical application. I kept steadily to the course of training duly prescribed. More and more, however, did the pure, rather than the technical, aspect of science, and its bearing on philosophical problems, appeal to me. That I suppose is why I chose the path of life I eventually followed.

For, when my year under Huxley was over, I stood at a parting of the ways. What next? Practice of the profession for which I held the requisite diplomas; or, in spite of my lack of specialization on any accredited line, to become a teacher—one whose voice, as I hoped (greatly daring), might perchance carry beyond the walls of classroom or lecture theater.

That, however, was only a possibility in the future. I must put my prentice hand to the test, while I did some pot-boiling work as assayer for a mining company. I tried short courses of lectures to see if I could hold an audience. I spent a year in part-time science teaching in a large private school. Then I got in the thin end of the wedge on my appointment as Lecturer in Physical Science, English Literature, and Constitutional History (all three, save the mark!) in the Diocesan College at Rondebosch near Cape Town. My chief work was with undergraduates in preparation for degrees in the Cape University.

Here I put in five years' service. Shortly after my return to England I joined, as Professor of Geology and Zoology, the small staff of University College, Bristol; saw, and perhaps (as a Principal for twenty-three years) contributed a little to the securing of a charter giving to Bristol university status; and stayed on till I was placed on the shelf of superannuation as Emeritus Professor of Psychology (1920).

IV

During my sojourn at the Cape (1878-1883), apart from my work as lecturer, which kept me pretty busy during term, I sought to set my house of thought in order.

First, there were Berkeley and Co. to be reckoned with in the light of further reading of what they had written and of what current scholarship was saying about them. What was their message to me? They were great architects in philosophy; and their material, from foundation to most elaborate façade, was ideas—using this word in a comprehensive sense for anything we can experience or think about as objective under reference. Even "feelings" are objectified in the field of thought-reference. Some of these ideas are built up into what we distinguish as the external world, which we speak of as experienced or known; others are built up into minds as experiencing or knowing. But the former, no less than the latter, are, insofar as known, 'in mind'—"by way of idea" as Berkeley put it. Therefore, said Berkeley, there is no physical world that has any existence save "by way of idea." That seemed to me to go beyond the evidence. There might be or there might not. It was open to me to accept either hypothesis, and to see how a constructive scheme works out. On evolutionary grounds, if no other, I believed, though I could not prove, that a physical world there is. But I agreed with Berkeley that, if not its *esse* (under hypothesis), still its *sic esse* (under acquaintance and knowledge) *est percipi.* In other words the physical world is a thought-construct plus the hypothesis that its *esse* is in some sense independent of its *sic esse;* and yet that they are in some way interdependent so that, in current phrase, the physical world is 'represented' in the mental realm of ideas.

I reverted to Locke and was up against primary and secondary qualities. The former were really in 'sensible objects' as part of their physical *esse;* the latter were imported by the mind as part of their mental *sic esse.* Even then I harbored suspicions—little more than notes of interrogation—that Locke had got primary and secondary wrong way up; that in my infant child, for example, secondary qualities *via* sense ideas (those of taste and color and the rest) come first, and that so-called primary qualities implied later mental importations involving spatial relations and so forth. It was, however, only a vague surmise to await further consideration when I had more facts to go on. In any case, I saw no escape from Berkeley's argument which led to the conclusion that, in mental regard, both alike fall under the heading of *sic esse;* that neither 'resembles,' though both 'represent,' the physical nature of things in themselves.

But then Reid bade me accept at its face value the direct deliverance of common sense which assured him that the rosebud out there continues to exist just as one sees it, whether one chances to see it or not. Of course, I had to admit that this was common belief with which everyone starts; and that things work out quite comfortably on this supposition. None the less, I mistrusted the 'intuition' on which it is said to be based. Was common-sense belief so simple an affair as Reid supposed it to be? Or was it a very complex affair, compounded of many coalescent factors which it is the task of analysis to disclose? I was beginning—but only beginning—to suspect that intuitions are always secondary and not primary, derivative and not original, compound and not elementary. Still, I realized that there is a problem. How comes it that a complex intuition (if complex it be) takes on that simple and unitary form which common sense finds and naïvely accepts in its simplicity? I could not make answer, I could not as yet see my way to an answer.

At all events, it seemed to me that one could not gaily step across from the idea of the rosebud 'in mind' to a physical thing in the external world which goes by the same name. Nor could one confidently assert that 'this,' in any strict sense, resembles 'that.' And it seemed to me that, the further one gets in the analysis of 'this' and of 'that,' the further one gets from any resemblance between the mental and the physical. Such, for me, was the message of Huxley in his discussion of the "Metaphysics of Sensation." But if one could not step across from 'this' to 'that,' could one get across from 'that' to 'this'? Here "that" was some physical transaction within the physical body—say somewhere in the brain. And to say that here there was aught of resemblance of 'that' and 'this' seemed well-nigh absurd. How, then, was the presence of 'that' in some way connected with the occurrence of 'this'? I was up against the body-mind problem. Concerning this I read all that I could lay hands on. Spinoza's 'identity hypothesis'—two attributes of one substance—seemed to be on the right track. Still, what, in terms of empirical science, was this 'substance'; what were these 'attributes'? Clifford's *tour de force* anent 'mind-stuff' intrigued me. But what was the relation between 'stuff,' in some sense, and 'substance,' in some sense? In a note of a little later date I find the query: "Are the atoms in a molecule its 'stuff,' and is what we call its 'substance' just the way in which they 'go together' in the molecule? Locke, speaking of substance, says that 'a certain number of simple ideas go constantly together.' Are not these simple ideas the stuff of mind; and is not their going together the admittedly complex idea of the substance of the mind? Do we, in molecule or mind, need other substance than this?" But how comes it that Clifford's mind-stuff takes on the appearance of matter-stuff? I was puzzled, as, so far as I could make out, were my betters.

Meanwhile, I wrestled with the problems of evolution. More and more did it seem that, for me, near the heart of them was the relation or co-relation of the physical, culminating in observable behavior as physiologically interpreted, and (at any rate in man and some animals) the mental, to which introspection in the sense of an appeal to one's own first-hand experience affords the sole clue, though we refer or 'impute' like kinds of experience to others, however we may interpret the fact that we have come to do so.

I reread Darwin with special regard to this problem; browsed in Romanes' *Animal Intelligence;* and resolutely tackled Herbert Spencer. Like others, I could not but be struck by Darwin's open-eyed facing of difficulties—"himself his severest critic"—as contrasted with Spencer's blind-spot to evidence which did not support his cherished convictions. None the less, I was caught within the sphere of his influence and believed that when the chaff was winnowed from the grain there was seed for a rich harvest.

With regard to Romanes' collection of anecdotes, psychologically interesting in its way, I felt, as no doubt he did, that not on such anecdotal foundations could a science of comparative psychology be built. Most of the stories were merely casual records, supplemented by amateurish opinions of passing observers whose psychological training was well-nigh negligible. I then entertained doubts whether one could extract from the minds of animals (wholly inferential from their observable behavior) the data requisite for a science, properly so called. Did one get out of the animal mind aught else than that which one put into it?

Still, here at any rate was material for a science of animal behavior, a purely observational science. And here an interpretation of instinct seemed to be crucial. Was it to be physiological only? Was there nothing mental about it? If there is something mental, how does the mind step in and what part does it play. Romanes spoke of instinct as reflex action into which is "imported the element of consciousness." In what way and whence was it 'imported'? One harks back to the body-mind problem.

In all this there is little more than was 'common form' in many young men of my day. But I am invited to be autobiographical. Even so, I must confess that what I have set down is too crisp and clear-cut. There was not a little mental wobbling. It must suffice to show at least the stage that I had thus far reached in my pilgrim's progress.

V

My first ten years at University College Bristol (1884-1893) were, save in vacation, fully occupied with the duties of daily routine. I published a little book on *The Springs of Conduct* as a *ballon d'essai* to ascertain whether my voice *would* "carry beyond the walls of my classroom and lecture theater" and to elicit helpful criticism. It contained a lot of poor stuff; but it served my purpose in writing it. I compiled also a textbook on *Animal Biology* and wrote papers on local geology. But I realized that in zoology and geology I was no more than a tolerably conscientious hireling; and, to be frank, I did not aspire to be more than this. I felt that if I was to contribute anything to the advance of knowledge it must be in the field of mental evolution; and the most promising corner of that field for further intensive culture seemed to be the lower reaches of mind near the contour line below which lies all that was conventionally spoken of as instinct, while above it is the region of intelligence. So my vacation studies took form in a somewhat portly work on *Animal Life and Intelligence.*

Still, though this gave, as I ventured to hope, a fairly extensive survey of the facts and theories which must be taken into consideration, it showed little sign of that 'further intensive culture' which comes only through close observation under experimental conditions. The next step, then, say in 1893, was to remedy this defect by getting to work upon some definite inquiry with a definite end in view.

It seemed that, broadly speaking, the characterizing feature of instinctive behavior—or, more strictly, the instinctive factor in behavior—is *like* performance on the first and on all subsequent occasions. If the performance is markedly different on subsequent occasions, some other factor, say 'intelligence,' has to be reckoned with. The first thing to do, therefore, was to get at first occasions and to make pretty sure that they *are* the first occasions on which some specific performance on the part of this or that animal is in evidence. In the case of insects there was pretty good evidence that the instinctive factor is predominant, and that performance on the first and on later occasions is very similar; though, even here, it is hard to say on what subsequent occasion, and under what conditions, some measure of intelligence may justifiably be 'imputed.'

I chose young birds of many species, hatched out in an incubator, as the chief, but not the only subjects for close, even meticulous, scrutiny—with results which are on record. In observations on older birds, and on other animals, such as dogs, I sought always, with regard to any performance, to take note of the whole sequence of occasions which led up to it. Here the aim was to ascertain how far, if at all, a 'rational' or reflective factor was in evidence. And here again the results are on record.

I wish now to emphasize that throughout the whole investigation, from first to last, my central interest has been psychological as I understand the meaning of this word. My aim has been to get at the mind of the chick or the dog or another, and to frame generalizations with regard to mental evolution. I could only do so through close observation of behavior. But, for me, the plain tale of behavior, as we observe and describe it, yields only, as I have put it, body-story and not mind-story. Mind-story is always 'imputed' insofar as one can put oneself in the place of another. And this 'imputation,' as I now call it, must always be hazardous. But we can in varying measure reduce its hazardry insofar as it may be checked by an appeal to one's own first-hand experience under introspection.

Introspection is a reflective process on our part. But it does not follow that what we *find* under introspection must itself be reflective. I find much in my first-hand experience which is unreflective, and lower still, subconscious. To interpret animal behavior one must learn also to see one's own mentality at levels of development much lower than one's top-level of reflective self-consciousness. It is not easy, and savors somewhat of paradox. One has reflectively to put off, not only one's reflective spectacles, but even one's perceptive spectacles, and get down to the bare sensory foundations of one's mental equipment. But it can in some measure be done; or, if it cannot be done, we can learn nothing of mental development in the individual or of mental evolution in animals or in men.

Dig down as deeply as I can to the substrata of my own mind—put myself in the place of another, say an infant or a newly hatched chick—I find not only items of mind-stuff analytically teased out, but substantial organization of that stuff in some sort of orderly pattern (Gestalt) ; and this organization is not physiological only—though that it is in the body-story—it is mental also at the very roots of the mind-story. Mind—even the lowest conceivable mind, say that of an amoeba—discloses some relational organization. And this organization is, under analogy, no mere mechanical mixture of aggregated particles of elementary 'stuff,' it is an organic compound with 'substantial unity.'

It may be said—it has again and again been said—that the line I have been led to draw between the body-story (with which physics and physiology are concerned) and the mind-story (with which psychology is concerned) implies a drastic separation reminiscent of the old notion of a 'great gulf' fixed between matter and mind. As I draw the distinction in scientific regard, that is not so. Events in the body are always, as I believe, so co-related with occurrences in the mind that with adequate knowledge one could infer these from those and those from these. This, as yet, may be only a working hypothesis. But if we can in no wise infer mental occurrences from observable behavior, anything like a genetic science of mind is in sorry plight. We are at the mercy of some sort of 'direct revelation through intuition' which itself needs psychological interpretation.

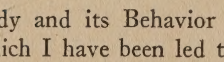
What I seek here to emphasize is that all experimental work in a psychological laboratory, however simple or however complex may be the instrumental means to its prosecution, has for its aim and objective the telling of mind-story. Mental relations are always in the psychological focus. The 'great gulf' is just the difference between mental relations, on the one hand, and physical relations, on the other hand. They are closely co-related; but under no speculative 'identity hypothesis' can *this* radical distinction be eliminated.

VI

It is fully thirty-five years since, through systematic observation under experimental conditions, I began to hammer out a psychological scheme (such as it is) which I could label my own. At the close of last century, in *Animal Behavior,* I gave expression to the form it had then taken. Already there were changes which rendered earlier statements out of date. Now I look back on it and see only in the making my outlook of today in *The Animal Mind.*

I suppose that an autobiographical account of the successive changes of opinion and attitude might be set down. I doubt whether in my case an attempt to do anything of the sort would be worth while. I propose, instead, to summarize my present position.

Anyone's interpretation of mental organization is in some way fitted in with his philosophical scheme. In detail, this scheme is probably a pretty complex affair. But one may ask him: What after all is the ABC of it? I ask myself this question, and put the answer, to begin with, in diagrammatic form as the triangle *ABC.*



Here *B* stands for Body and its Behavior as part of the physical world in the *esse* of which I have been led to believe. *C* stands for someone's Conscious experience—not only my own, of which I have no doubt, but that of minds other than mine in the *esse* of which I have been led to believe, though in some minds the word "conscious" (if it be retained) must be written with a very small '*c.*'

A stands for Activity or Agency—some driving Force, or set of Forces—in terms of which I seek to explain or account for all that happens in *B* and *C*. In the *esse* of this, too, I have been led to believe. I leave these beliefs as they stand, feeling pretty sure that everyone has passed through a stage of sharing them in some form.

Now in my scheme, psychology is concerned with *A* or with *B* only insofar as there are, within *C*, 'ideas' which represent them. In other words, on *this* scheme, the psychologist deals with *B* or with *A* (and, indeed, with other minds than his own) in terms of 'ideal construction'; and how such ideal construction takes form in reflective thought, it is part of his business to ascertain—if he can. He is faced by an evolutionary problem. Ideal constructs there *are* in the reflective field of view. He must render some genetic account of how they got there.

But on these terms the ideal constructs which 'represent' *A* and *B* are within *C*. Psychologically, we are dealing with $(A' \; B') \; C$ where the parentheses enclose reflective ideas of *A* and *B*.

My philosophical assumption, however, is that there is a 'real' *A* and a 'real' *B* whose *esse* is independent of ideal constructs within *C*. This is an assumption which goes outside or beyond psychology. Berkeley did not accept such an assumption with reference to *B*, but fully accepted such an assumption with reference to *A*. I accept both. This means that, outside of the subject-matter of psychology, there is that of physics; and that beyond that of psychology and that of physics there is that of Activity. The words 'outside' and 'beyond' have here no spatial implication. They signify "extraneous to the *C* universe of discourse."

The salient point for emphasis is that what lies beyond physics and psychology—namely, Activity or any form of driving Force—shall, on methodological grounds—that is, as a convention or policy—be regarded as "external to the province of science." Here I follow Huxley and Clifford. Many physicists, so long as they keep within their scientific domain, accept this policy, and only invoke Force (in the mediaeval sense of this word) when they divagate into philosophical discussion. Some psychologists do so on like grounds. I am among their number as a matter of policy.

If, then, in science, as thus delimited, one strikes out *A*, one is left with *B* and *C*. It follows that not only has one nothing to do with driving Force within *B* or within *C*, but nothing to do with 'interaction,' reciprocal or one-sided, as between *B* and *C*. One says only that *B*-events and *C*-occurrences are co-related, or are concomitant within the living organism as hyphened body-mind.

Still, one freely admits—nay, strenuously contends—that what is co-related in *B* and *C* is this or that mode of organization. These modes of organization one accepts as one finds them without (in science) asking: To what organizing Activity are they due?

Evolution, from the scientific point of view, is progressive organization, 'this' in *C* co-related with 'that' in *B*. To distinguish a special feature of organized advance, alike in *B* and in *C*, I have of late borrowed from G. H. Lewes the word 'emergent.' But the notion it embodies is quite old. Briefly stated, the hypothesis is that when certain items of 'stuff', say *o p q*, enter into some relational organization *R* in unity of 'substance,' the whole *R* (*o p q*) has some 'properties' which could not be deduced from prior knowledge of the properties of *o*, *p*, and *q* taken severally. So far, the advance is relatively step-like or 'jumpy.'

Save for laying stress on the fact that this purports to be a scientific hypothesis, with no reference, as such, to creative Activity, I need here say no more than that I accept it. But to indicate its psychological bearing, and to show that it is nowise new, I quote from Professor Höffding this passage. Speaking of James Mill, Höffding says that "he lays great weight on the point that . . . several ideas and feelings may enter into so intimate a union with each other as to become inseparable, while the new totality, thus formed, possesses qualities which are not possessed by any of the parts. . . . The new qualities of the product cannot be deduced from the factors."

I have tried to state as clearly as due regard for brevity permits my philosophical ABC. I have done so because, as I see the present state of affairs, there are three main schools in psychology.

1. Those who include *A* as a scientific concept and explain mental occurrences as due to the Causality of Mind, or perhaps in terms of driving Forces of which sundry Instincts are specific examples.

2. Those who exclude *C* from the pale of science and define psychology as "whole action physiology" and no more.

3. Those who agree with advocates under (1) in claiming for psychology the status of a branch of science whose proper domain is mental organization, but who differ from them in relegating to

philosophy all discussion of the further question: To what Source—creative or directive—should we attribute these modes of organization?

I serve in the ranks of those who belong to the third of these schools of psychology.

VII

Physicists claim that they deal with a closed system of physical events intolerant of 'psychic additions.' I suggest that, in like sense and on like grounds, the psychologist may deal with a closed system of mental occurrences. In each case the science is professedly 'abstract.' Psychology treats of mental relatedness in abstraction from physical relatedness, co-present but taken for granted. That is what I mean by urging that we should distinguish mind-story from body-story—distinguish, be it noted, not separate.

Practically, no doubt, we deal with matters 'in double regard'—as physicists (and physiologists) *and* as psychologists. We deal with mind-story as connected with, and not divorced from, body-story. None the less, the one should be distinguished from the other since the relations are of a different order.

On this understanding I elect to use the word 'behavior' with reference to body-story. It denotes organized sets of events in the physical world. It is *B*-business and should be observed like any other mode of *B*-business. So far, in reference to body-story, I am strictly 'behaviorist.' Then I use the word 'instinctive' (with due warning) as adjectival to behavior, accepting the common usage of this adjective by naturalists. Instinctive behavior thus falls under the heading of body-story. But it has its accompaniment or concomitant in mind-story. That I speak of as 'awareness in behaving.' In my use of the word 'awareness' (again with due warning) I depart from current usage. I do not now speak of awareness *of* something seen, tasted, or touched. I restrict the word 'awareness' (followed by *in*) to some mode of so-called 'subjective' experience, such as behav*ing*, see*ing*, tast*ing*. For that which is objectively experienc*ed* I use the word 'reference' (followed by *to*).

Now suppose that, for the first time in its life—on some first occasion—a young animal, or an infant, behaves in some highly organized way. This affords an example of instinctive behavior. It is an instance that falls under body-story. The performance, however, is accompanied by awareness in behaving; and the total awareness in behaving is no less highly organized, as such, in mind-story than is the behavior in body-story. The intrinsic 'ground' (as I put it) of the behavior is the inherited organization of events in the body—to be discussed under physiology. Concomitant with this is the intrinsic organization of awareness in behaving which is part of mind-story.

But there are also external 'conditions' to be reckoned with in body-story—namely, those of sensory stimulation. Here the mental accompaniment is (*a*) awareness in sensing—seeing, tasting, and so forth—and (*b*) the evolutionary precursor of that which in later development takes form as perceptive reference. Here I have been in difficulties. Owing to the ambiguity of the word 'sensations,' I venture to speak of (*b*) as 'percipient' reference though the word 'reference' is used in the literary figure of prolepsis, since percipience as such affords only the data for reference under perception. Percipient reference in this attenuated sense is a highly organized 'pattern' in mind-story (even on the first occasion), answering to the highly organized pattern of receptor stimulation, and its neural outcome, in body-story.

This synthesis of $a + b +$ awareness in behaving is all that I feel justified in imputing to a mind when 'brother body' is at the instinctive stage of behavior. And, insofar as the behavior is instinctive, that mind is shut up within the passing moment ('specious present') of the current occasion. There is, as yet, no location of 'objects' in space; no reference to foregoing occasions in the past; no reference to occasions that may come in the future.

One has to realize—only lately have I been led more fully to realize—how the evolutionary advance from the lower percipient stage of mental development to a higher perceptive stage, and the further advance to a yet higher reflective stage, opens up for genetic psychology a problem in that which one may speak of as 'mental space and time' in an objective field of reference. Taking our cue from the outcome of modern physics we are prone to suppose that even the most primitive mind starts with ready-made ideas (in the Lockeian sense) of space and of time. (The concept of space-time is quite recent.) We are prone to suppose, for example, that when a newly-hatched chick pecks for the first time at a rice-grain out there, he 'must' have in mind an idea of its 'thereness.' In order to peck at it there, we say, he must 'know' in some intuitional fashion where it is. This he has not progressively to learn.

The alternative interpretation of the observable facts is that he *has* to learn this. Crudely stated, he does not know that it is there and then find it there on pecking at it. He finds it there through instinctively pecking at it, and thus learns its thereness for further behavior. The so-called direct and primary (nativistic) intuition of space is, as I believe, a secondary or derivative compound, not an elementary factor which is primary or original.

On this interpretation how does perceptive thereness arise in the course of genetic process? One has sensory percipience, say under vision; one has also awareness in behaving thuswise. When, on subsequent occasions, these *combine* in remembrance there arises, or 'emerges,' a new quality referred to the situation under associative organization. Certain sense-ideas and certain behavior-feelings and their revivals enter into so intimate a union as to become inseparable, while the new totality, thus formed, has the new quality of thereness which could not be deduced from those of the factors taken severally.

I must be content here thus briefly to state, and not attempt to defend, this hypothesis with respect to the genetic origin of perceptive thereness in space. My autobiographical aim is to show the later drift of my thought.

Not only space, however, but time also, dawns on the perceptive horizon. When, quite early in one's infant days, one has learned to see a thing there, one 'expects' on further behavior to find it there—or, as I prefer to put it, one has 'fore-experience' of its thereness. I regard fore-experience as the chief avenue through which is opened up the first dim vista of that which we reflectively speak of as time.

Revert to what I speak of as the percipient stage of mental development. Assume that the primitive mind is carried forward on the crest of a wave which advances through a series of 'nows,' each of which is a specious present. *We* interpret this in terms of a space-time frame, cunningly devised to enable us to record, and to deal metrically with, the passage of events which is represented therein. But do we credit the primitive mind with anything of this sort? One must put oneself in *its* place. It is shut up in the now of what *we* call the specious present—a short span of *our* time-plan of events. Let us further assume on the basis of our first-hand experience that each percipient occurrence does not snap out instantaneously but leaves at the rear of the passing moment what we may picturesquely speak of as a fading trail of the past. There is mental 'duration' answering to physical and physiological 'endurance' of events. If so, we have something 'timey'—some time-change—to start with, even at the percipient stage of mental development. We speak reflectively of change occurring in time. Here we invert the statement and say that time-experience arises through change. Hence, if we include not only strictly instantaneous occurrences but their fading trails, the range of current experience is so far widened.

But, so far, in the mind as percipient only, there is no hint of what is coming—or, as I put it, no glimmer of fore-experience. For awhile I spoke of this as 'prospective reference.' Since what I meant has been often misunderstood, I now try to make matters clearer by distinguishing fore-experience from prospective reference. I want to get at something which, as I think, may be inferred from the behavior of a chick in the first day or two of its life after hatching. After pecking two or three times at some nasty thing and experiencing its nauseous taste, he thereafter seizes it no more. Why is this? I assume, on the basis of my own first-hand experience on analogous occasions, that he has in mind fore-taste, though there is no actual taste through the stimulation of receptors in the mouth. In like manner, his later behavior in many ways and respects may be interpreted if one may impute to him varied modes of fore-experience as the fainter revival (with a difference) of precedent modes of more direct experience.

The emphasis here falls on the fact—if fact it be—that fore-experience, as such, falls within the specious present or passing moment of some *this* occasion as what William James called a fringe of futurity in its advancing edge. It does not, like prospective reference at the later stage of reflection, look forward to *subsequent* occasions separated by a time-gap from that which is now present. In this sense, that which is for us the future is not for the percipient mind opened out.

I venture then to say—though the statement is elliptical—that perceptively intelligent behavior is under the *guidance* of fore-experience. I here use the word 'guidance' in a purely relational sense, and not, as others may use it, with the implication of some directive Activity to which it is due. In matters of science *such* Guidance is beyond my purview. Surely one does not depart far from accredited usage if one says that fore-knowledge with prospective reference to future events such as those of an eclipse, or, at a lower stage of mental development, fore-experience, as above defined, affords guidance in reflective conduct, on the one hand, and in intelligent behavior on the other hand.

VIII

My aim is autobiographical—just to indicate the pattern of my later thought, and my present psychological outlook.

On my interpretation, there is at the percipient stage of mental development—co-related with the instinctive stage of observable behavior—neither mental reference to spatial thereness, nor fore-experience as a guide to behavior. It is with the advent of thereness and of fore-experience that we pass in mind-story from percipience to perception, and in the co-related body-story from instinctive to conditioned behavior. At least I incline to the opinion that 'conditioning' may be taken to imply some tincture of fore-experience. In any case, it is probable that the advance of intelligence in the higher animals, and in the infant, in large measure consists in the progressive organization of thereness and of fore-experience in the course of individual life.

But, however complex this associative organization may be—however nicely attuned to the circumstances of the physical environment represented in perception on the part of animal or infant—it is still a far cry from this to the reflective organization of a space-time *plan,* applicable at first to some group of situations, more or less similar and yet more or less different; applicable at last to all situations in any known context. Supervenient on perceptive organization there is in some animals (perhaps only the higher apes) and in every normal child reflective organization. Thus, broadly speaking, I distinguish at least three salient stages of advance in mental organization—percipient, perceptive, and reflective; subconscious, conscious, and self-conscious, respectively.

I am old-fashioned enough to emphasize the *self*-conscious character of all reflective procedure. By this I mean that in this procedure there is always the idea of self (one's own self or that of another) in the picture. I doubt whether a merely perceptive animal or infant has any idea of self in his unreflective picture.

But my present concern is with the reflective 'picture,' within which the self plays at any rate a prominent part. It implies always a space-time plan of physical events and mental occurrences—a plan in which any 'there' and any 'then' has reflectively realized relations to the central 'here and now.' The threefold stress is on *plan* in mind, on *relation,* as *conceived*—or, if perceived, subject always to reflective backing. My position will be scouted by many of my colleagues as untenable. But I believe that not until the stage of reflection is reached do relations swim into the ken of mind. It may be asked: Are not thereness and fore-experience relational? Yes; unquestionably, *as we reflective folk* interpret them. But the rabbit that 'goes for' a carrot there, and is guided in doing so by present and insistent fore-taste, may be quite incapable of framing any idea of relations if there be in his mind no reflective backing. Here, however, I make no attempt to defend this position, namely, that, prior to reflection, ideas of relation are not yet in being. I am content to state it as an autobiographical confession.

They do so, however, when the level of reflection is reached. Let us, then, fix our attention on temporal relations of before and after. Then we have anticipation and retrospection. We say that there is prospective reference to some future occasion or retrospective reference to some occasion in the past. But I submit that there is also, as reflective backing, present reference to a time-plan in which these future occasions not yet in being, or these past occasions no longer in being, are reflectively charted. We deal with the map (within the specious present) on which the milestones ahead of us and behind us (and intervening occurrences) are represented to scale. We, so to speak, view a picture of the course of affairs through reflective spectacles on which this time-scale is recorded. In forward regard there is not only fore-experience adjunct to *this* current occasion but prospective reference to some *future* occasion duly entered on the time-chart—say an appointment for 10:30 tomorrow morning. Here self (myself) is clearly in the picture. In the case of some astronomical event charted to occur a dozen years hence, I am not in the picture. But, I take it, one normally thinks of some other self, some observer, who, were he so disposed, could witness the event and confirm the accuracy of prospective reference thereto. It is in this sense, I submit, that there is always the idea of self—not necessarily oneself but often some other self—in the reflective picture. And I think that in the three-year-old child one can see evidence of the dawn of such self-consciousness.

I question, however, whether the ox, or even the dog (or the infant) ever 'thinks of' some future event with prospective reference, or of some past event with retrospective reference, having the while an idea of self—himself or another—in the reflective field of view. In that sense, at the perceptive level of mental development there is no self-consciousness and no time-plan to which past or future occasions are referred.

Obviously the vexed problem of the genetic development of retrospective memory and of prospective anticipation is thus opened up. I cannot enter on it here. Suffice it to say that, even at the percipient level, we must impute to the primitive mind (of a newly hatched chick, for example) 'retentiveness' as the persistence of mental organization thus far established. At the perceptive level we must impute also 'remembrance,' in the sense of such revival as takes form as fore-experience. Only at the reflective level *need* we impute prospective reference to future occasions or retrospective reference to occasions in the past.

If we restrict the use of the word 'memory' to the last phase, then infants and most animals have no memory but plenty of remembrance. If we emphasize retentiveness, then there are no animals, even the lowest, that have no memory in this sense. In any case—however we define it—we should, as psychologists, discuss memory in terms of mental relations under (*a*) awareness in remembering or recollecting, and (*b*) reference to that which is remembered or recollected. We should keep within our province of scientific inquiry. And within that province we should not regard Memory as an Activity to which such organization as we find may be due. In the science of psychology we should not assign to Memory the rôle of Efficient Cause or to Anticipation the rôle of Final Cause.

IX

If one may distinguish at least three salient stages in mental evolution—provisionally named percipient, perceptive, and reflective (subconscious, conscious, and self-conscious), and if, under imputation, one may assign this or that level of mentality to an animal whose observable behavior has such and such a character, or whose procedure passes through such and such stages, one has a definite scheme as a basis for interpretation. And, since this scheme is professedly genetic and evolutionary, one should not on this basis interpret a lower form of behavior as implying a higher level of mentality than the evidence demands. In evolutionary and developmental regard one should proceed from below upwards.

But imputation of this or that mental status to a mind other than one's own should always be endorsed by introspection which deals with one's own first-hand experience. Here one proceeds from above downwards, starting as interpreter from the top-level of reflection. Critics of introspective procedure are prone to contend that it cannot pierce below this reflective level at which it must start. If that be so in their own case, it would be impertinent to assert that for them it is not so. There are, however, others who find that in themselves affairs mental are going on *at all three levels*. In some affairs they act reflectively; in many others they are behaving unreflectively, and are, as they aver, unmistakably aware in so behaving. A space-time plan and reference to self then drops out of the introspective picture. But fore-experience may be quite recognizably there. Even this may not be in evidence. Multifarious occurrences just come without any warning in fore-experience that one can detect, and bring something quite new and unexpected.

No doubt these unreflective factors in one's mentality are introspectively described through reflective spectacles. But I think that allowance can be made, in large measure if not wholly, for the distorting influence of these spectacles. Otherwise, if it can nowise be reached by introspection, one has not first-hand acquaintance with and knowledge of unreflective procedure as it runs its course unaffected by reflective coloring.

The question, however, which I wish here to raise is whether we should, as psychologists, interpret any mode of unreflective procedure in terms of teleology. I, for one, as at present advised, should not do so. Psychologically, the procedure which I should designate as teleological always implies an end in view to be attained on some future occasion if all goes well. It implies not only fore-experience adjunct to some current occasion but prospective reference to a later occasion charted in a reflective time-plan. In reflective procedure on our part, prospective reference with teleological relations, as thus characterized, is very much in evidence. But is it in evidence in unreflective procedure? I think not. Present fore-experience, as distinguished from prospective reference, is all that the evidence demands. And prospective reference is distinctive of a mind which has reached reflective status.

I here accept what I take to be a legitimate and strictly psychological definition of the word 'teleological' as adjectival to procedure that implies a mental relation which obtains when one has as end in view some specific change in an existing situation to take effect on some future occasion to which there is prospective reference; or to reproduce this existing situation on some future occasion. There is a temporal relation between an earlier and a later phase in the course of one's passage from precedent end in view to subsequent outcome in fulfilment. In the earlier phase one's procedure is teleological with prospective reference to the later.

I am, however, well aware that a dictionary definition of teleology is: "a Doctrine of Final Causes." But I am not alone in contending that a doctrine of final causality (and indeed of all causality as distinguished from relational causation) lies beyond the purview of natural science. It should be relegated to the philosophical classroom where it is properly in place.

This philosophical question with regard to final causality I do not here raise. The psychological question I do raise is whether unreflective procedure in men and some animals should be interpreted in terms of prospective reference on their part to some anticipated outcome. And I add the further questions: Is it not often interpreted in such terms? If so, does not this run counter to the evolutionary canon that we should not interpret an earlier and lower stage of mental development in terms applicable only to the interpretation of the higher and later stage?

It may, of course, be said that this so-called canon of evolutionary interpretation must be rejected. I have been led unreservedly to accept it. That is where an autobiographical confession of considered belief comes in.

How then, in summary statement, do matters work out?

There are three stages of evolutionary and developmental advance—percipient, perceptive, and reflective. Only at the last of these stages are there teleological relations. Hence we should not interpret in mind-story (or in co-related body-story) anything that happens prior to the advent of reflection in terms of teleology.

This statement, however, does not purport to have any reference to the philosophical concept of Final Causality. Teleology in that sense is reserved for discussion, not by psychologists in their capacity of men of science but by those who, as M. Bergson puts it (in one passage), "superpose on scientific truth a knowledge of another kind which may be called metaphysical."

X

I must now draw to a close. I started in my 'teens with Berkeley. In his *ABC* there are God, the material world, and the self-conscious subject. He acknowledges the *esse* of *A* and of a great number of *C*'s (other than himself). He does not acknowledge the *esse* of the material world as such. It has being only 'by way of of idea' under *percipi*. In my *ABC* the *esse* of *A, B,* and *C* (other minds than my own) is acknowledged in an attitude of belief. The discussion of *B* and *C* (and of their co-relation) is reserved for science. That of *A* is relegated to philosophy. Physics (including physiology) deals with *B* as a closed system of events which brooks no 'psychic additions.' Psychology deals with *C,* including, of course, *A* and *B* insofar as they are disclosed 'by way of idea,' under *concipi*. It, too, is a closed system. But 'closed' in what sense? In the sense that it selects as its province of inquiry mental relations as distinguished from physical relations. But in the concrete—in the living organism and its environment—there are both physical and mental relations. Hence the need for inquiry into the ways in which they are co-related. This inquiry is prosecuted by physicists and psychologists when they meet in joint session, and discuss matters 'in double regard.'

Experimental work in a psychological laboratory is joint-session-business. Physical apparatus is more and more cunningly devised; nuances of bodily behavior are more and more delicately observed and recorded; a physiological interpretation in terms of external stimulation (or internal excitation) and response is kept steadily in view; adaptation of behavior to circumstances is fully considered. But to what end? I submit that the end in view is just psychology—the science which deals with mind. All the rest is means to that end.

No doubt, this science may be applied to the attainment of some further end—some 'practical' end, say in the work of the teacher or in industrial affairs. But this is applied psychology—analogous to applied physics in engineering, or applied chemistry in brewing, or applied biology in cattle-breeding. I want to get down to bed-rock in the 'pure' science of psychology. Then we have a closed system of *C*.

Does this mean that all the current work in applied psychology—in medical psychology, for instance—does not count? Far from it. All counts; but, irrespective of application or social value, it counts in pure psychology as means to the end of forging a science of mind—abstract, no doubt, just as pure physics is abstract, but a factor in the concreteness of natural science as a whole, just as natural science is a factor in the concreteness of philosophy as a larger whole.

Thus we come back to some *ABC* which may represent schematically the concrete whole of philosophy. Here the methodological abstractedness of science is superseded. And in accordance with the 'organic principle'—if that be accepted—this whole is more than the aggregated 'stuff' of its several constituent factors. It has 'substantial unity' in their combined relatedness to each other. Neither *B* nor *A* is quite what it is save in its relation to *C*. And *C* is not all that it is apart from its relation to *A* and *B*. For philosophy, *C* is no longer a closed system.

Is this true also of *B*? That is still subject-matter for debate. I so far follow Berkeley as to believe that it is true also of *B*. Alike in perception and for reflective thought there remains always some tincture of *sic esse*.

And what of *A*? This again is a moot question. If one can find Activity in first-hand experience, one has there at least an instance of *A*. But, even so, does *A*, as universal throughout nature, wholly tally with this necessarily personal instance? Or must one rest content with the *sic esse* of ideal construction?

In response to editorial request, I have—with some diffidence in respect to my right or my capacity to place on record anything which my colleagues may deem it worth while to read—contributed my mite to this "History of Psychology." If I have mingled too much philosophical flavoring, I plead in excuse that it must e'en be my autobiography.

WALTER B. PILLSBURY

The determination of why any decision is made in life is very difficult. It is the more difficult when it is so complicated as the choice of a life career. To answer the question forty years or so after the choice is made blurs the answer still more. One can make up many reasons ranging from lack of any good opportunity to go in any other direction to a strong natural bent plus one or several of the strong stimuli applied in the formative period. So far as I can remember the first specific assertion of the decision in my case was made at the age of fourteen, when still in the second year of high school. I had chanced upon a copy of Carpenter's *Mental Physiology* in my father's library and had read it with great interest. I remember saying to my father as I finished the book that I would specialize in psychology when I grew up. How seriously the remark was meant is a question. Until well on in my college course I was supposed to be working towards the law. That also was not especially of my own initiative, so far as I can remember, but was rather acquiescence in the family opinion. I had not chosen any courses with special reference to either career, up to the last year of my college course.

The first really serious interest came from study under Professor H. K. Wolfe at the University of Nebraska, where I was a student from 1890 to 1892. Whether the course in psychology was required or whether I chanced into it, I do not remember. I do remember vividly the interest that was aroused by the subject and by the man. Wolfe demands a large place in the history of psychology for the number of men whom he led into an interest in the subject. He was one of the early students of Wundt. He had gone to Leipzig immediately after graduation from Nebraska, and had responded to what was best in the German environment. On the completion of his work for the degree, he went back to Nebraska as Professor of Philosophy, set up a laboratory and, alone, undertook to duplicate a large part of the work he had been doing at Leipzig. He gave a course in psychology, which ran through the year, and for which he required considerable work in the laboratory. In addition, he had several courses in philosophy, and did what work was done in education. He was an indefatigable worker, but gave so many courses and spent so much energy on them that he had no time for publication.

Wolfe's influence was exerted personally as well as in the classroom. He was always available for conversation, as he usually sat in the room where the reference works were, and seemed ready to break into his reading whenever a question came up. His conversation was as frequently general as technical. He and his family were interested in the Populist movement at the time and he was ready with comments on state politics, and discussion frequently drifted in that direction. He was as advanced in his interests in politics as in philosophy or religion. His viewpoint in psychology was liberal. He was more anxious that his students should think than that they should hold any particular point of view. He was a firm believer in experimentation, and made a session a week a requirement for each student in the elementary course. So far as I remember, he belonged to no special school. As texts he combined James and Ladd, and was, I presume, a fairly close follower of Wundt. That facts should stand for themselves was his dominant doctrine. One of the class exercises was to bisect a strip of paper, when held horizontally and vertically. Each student made a number of these bisections and the results were carefully kept. This exercise was continued through his whole career. The results were worked up after Wolfe's death by Guilford and published. This was, so far as I remember, Wolfe's only contribution after his Leipzig days. A respect for experiment, a belief in a scientific psychology, and a desire to see thinking for its own sake more general, were some of the gains from the two years with Wolfe.

My actual transition to work in psychology was due to a suggestion from Wolfe that I try for a fellowship in an eastern university. It came the year following my graduation while I was teaching mathematics and a general assortment of school subjects at Grand Island College. I was awarded a scholarship at Cornell for the year 1893-1894 and began my work in Titchener's second year. The laboratory was composed of five rooms transformed from classrooms. Titchener had no assistants and few graduate students. Miss Washburn was a fellow and a student in her second year. Titchener did not give the beginning course then, so had more leisure for advanced classes and for directing research. He was also still close to Wundt's position in general theory, so that there was little change in standpoint. Experimentation was the keynote in his teaching. Personally, Titchener was very unlike Wolfe. He held aristocratic opinions on most subjects as opposed to Wolfe's extreme democracy. He believed, while the teaching burden had compelled Wolfe to give over all writing, in publication as the end of the scholar's endeavor. He once said that the only certain immortality was the immortality of the printed page. He was American editor of *Mind* and continually made small contributions. He had also formed his connection with Hall and the *American Journal* which he continued until Hall's death. He kept the ideal before his students.

As a means of becoming acquainted with the larger trends of philosophy and psychology, the contacts at Cornell were immediately stimulating. As a scholar it became my duty to write abstracts of literature and to review books. This insured keeping up and extending a knowledge of German, and of my more rudimentary French and Italian, and also gave a greater immediate acquaintance with the foreign work in psychology than would have been gained in any other way. An exact evaluation of just what was derived from specific instruction in the four years at Cornell is very difficult to estimate after so long a time. The respect for experiment was certainly deepened; added to that was a greatly increased knowledge of what wide scholarship meant, and an esteem of its importance when added to investigation itself.

There was probably little to affect the tendency to one school or another. Titchener was still a devoted follower of Wundt, and none of the other schools had developed to the point of arousing antagonism. The most prominent whipping boy then was the old faculty psychology and the soul psychology of the religious schools. Both were even then sufficiently out of the picture to arouse no great warmth in the attack. Titchener was unwilling to take James's work quite seriously as psychology. He thought of it as literature and philosophy. He used Sully, which was new then, as a textbook in his long advanced course until he had translated Külpe. He did not, however, oppose his school to James's in any way. It was only later, in fact after my time at Cornell, that he first labeled himself a structuralist. Outside of psychology, a course in the logic of Bradley and Bosanquet had the most influence upon my later thinking. The treatment they gave of meaning, especially the sharp distinction between meaning and image, proved fruitful. Most important was the close personal contact with Titchener. This was especially true of the last two years, when I had a room adjoining his in Cascadilla, a ramshackly school building that had been taken over by the University and transformed into an apartment for faculty and students. As an assistant during these same two years, I had much experience in the preparation of apparatus under direction and in planning courses. This proved invaluable in many different ways.

The first experimental work published was a study of the methods of localizing a point upon the skin. The idea came while acting as a subject for Miss Washburn, who was then preparing her dissertation upon tactual localization. It was noticed in her experiments that the observer had a tendency to visualize the spot touched before an attempt was made to indicate where it was with a pencil held in the hand. We wondered how far that alone was accurate, and how far it needed to be corrected by the comparison of the two contacts in the trial and error search for the point touched. The test was made by localizing the point upon a photograph of the wrist. The results indicated that visualization was active, although it was not so accurate as the localization upon the skin. Both cooperate in the localization, although the comparison of contacts was apparently the more important factor. By a striking coincidence, Henri published a dissertation on the same subject, for which the work was done at Leipzig, just before we published. The two pieces of work were begun independently.

The choice of a topic for the dissertation was chosen in consultation. Titchener asked if I had anything I was particularly interested in, and I mentioned one or two minor problems, which were shown to be inadequate. Finally I settled on the problem of the mental processes in reading. The suggestion came from a single case of illusion. In the early morning light once I was looking for a house number, and saw on a letter box a badly scrawled MAIL. This was misread as a number, and I had started on when a second glance showed the mistake. The problem I set myself was to determine the relative importance of the sensations and of the memories in the development of the perception processes. An indication of the contributions of more subjective elements was sought in the amount of supplementation that was possible without discovering the error. Words were typewritten with certain letters omitted, others replaced by other letters and some had the letter blurred by printing an *x* over it. The letters were photographed and projected one at a time upon a ground-glass screen. On the projection apparatus a photographic shutter was attached so that a 1/5-second exposure could be given at will. The observers were merely asked to say what letters were seen and to describe any peculiarities that were noticed.

The experiments showed that considerable changes could be made, and have the word read, in many cases without noticing that any changes had been made in the letters. A study of the errors made possible an analysis of the procedure in building up a word under the ordinary conditions of reading. The importance of different sensory factors was studied directly from a study of the effects of changes in disturbing the reading operation. If the misprint was early in the word, the chance that it would be seen was greater than if it came in the middle. The last letters of the word also seemed to be important. The fact that omission of a letter was more readily noticed than blurring or changing one seemed to indicate that the general form of the word was an important element in determining what should be read. This was confirmed by the reports of the observers, who said that the change in the length was the first factor to impress them.

The more important part of the conclusions dealt with the laws of supplementation. This part of the work was done under the influence of Wundt's distinction between internal and external associations and between associative and apperceptive connections. It was assumed that the process of perception consisted in arousing retained elements that had been connected with the letters seen and the general form of the word. These constituted the associative connections. More stress was laid upon the factors which selected the associates. The experiment planned to bring out these forces by changing the general setting. For this purpose, in one series of experiments, a word associated with the word to be shown was called just before the word was exposed. The percentage of correct readings under these circumstances proved to be much greater than it was in the series in which no word was called. It also happened that, if the observers understood the word called in a different way from the one intended, they would at times see an entirely different word from the one given them, or at least entirely different from the one that was supposed to be shown in blurred form on the screen.

It was also noticed that chance changes in the attitude of the observer were very important in determining how the word would be read. Thus, Titchener would frequently come to the experiment from his office adjoining, where he had interrupted reading German or French. On several occasions he saw combinations that were peculiar to German in the English words that were shown. Similar confusions with the French also occurred, although these were less striking. Words that the observer had been on the point of using or that were related to his thoughts were also likely to influence the perception processes.

The theoretical interpretation of these results anticipated in some degree the conclusions of other workers later. This was before Külpe and his students had carried on the investigations which led to the development of the notion of *Aufgabe* on the direction of attending and on the course of associations in recall. Several of my conclusions stated approximately the same facts in a slightly different way. The formulation in the thesis was the basis of the classification of the conditions of attention as outlined in the later book on attention. In the dissertation I divided the factors that determine the way words would be read into objective and subjective conditions. By objective was then meant the effects of the letters actually seen, of the length of the words, and the different kinds of mutilations as they affected the form of the word. Under the subjective were included the associations between the letters seen and those that were recalled, and between the form of the whole word, and the image of that word. More nearly corresponding to the subjective conditions, as the word was used later, was the series of effects of the word called, the chance antecedent conditions and the other words in the series, the occupations of the preceding hour, etc.

More generally, a discussion was given of the way the word apperception had been used by earlier writers and was used at the time by Wundt. The dissertation was entitled "The Reading of Words; a Study in Apperception," which afforded an excuse for the more theoretical discussion. It was shown that apperception had been used originally to indicate the degree of clearness in consciousness, was then changed to mean the interaction between conscious processes that was according to Herbart the cause of the clearness, and, finally, Wundt had kept all of these meanings and added the suggestion of an active force or will in the popular sense. It was pointed out that so many different uses destroyed the value of the word. It was suggested that, if it was to be kept at all, it should be used to designate the fact that all elements of experience, past as well as present, were acting upon each other at any time. It should be the name for an observed interconnection and not for a force. The suggestion was made that Wundt could be interpreted to mean something of this kind and used apperception as the equivalent of will only for the sake of brevity. This use of the term and the general notion of interaction of past on present was used in the *Attention* several years later.

A year spent at Cornell after the completion of the work for the doctorate gave an opportunity to extend a knowledge of related sciences that were especially weak. Although still an assistant, I gave an independent course, and was more active in the teaching. The following year I was given a chance to go to Michigan and start a laboratory. I accepted in spite of being offered a chance to remain at Cornell as an instructor. At Michigan I was attached to the Department of Philosophy, but was given full charge of the laboratory and of the work in psychology. After a few years, Professor Wenley had me appointed Director of the Psychological Laboratory, although still attached to the Department of Philosophy. This arrangement continued until Professor Wenley's death in 1929, when a separate department was created, of which I was made chairman.

The early years at Michigan were marked by hard work, as there were large elementary classes, and, from the beginning, a few advanced students who took a disproportionately large share of the time. The latter time was gladly given. Almost from the beginning, opportunity was taken to extend acquaintance with the work of the biological sciences. Courses in nervous anatomy, given by Professors McMurrich and Huber, were followed. Little opportunity for work in that line was afforded at Cornell in those years. I also took advantage of a chance to begin a piece of work in physiology with Dr. Lombard. We studied the changes in circulation in connection with respiration and allied processes, especially the so-called Traube-Hering wave. Two papers were published as the result of the work. It gave me an opportunity to become better acquainted with physiological methods of recording circulation and with many ingenious devices that Dr. Lombard had developed in his laboratory.

The early work of my students was influenced by this physiological excursion. Several were devoted to a study of the so-called attention waves. Many investigators had noticed that, when they attempted to watch continuously some faint light, it would appear for a time and then vanish. Numerous attempts had been made to explain the phenomenon. The work with Dr. Lombard suggested that there might be some relation between the regular recurrence of these most effective periods in perception and the Traube-Hering waves, which showed, we found, in the pulse rate and in the volume of a member.

The duration of the two waves was not very different, and it seemed possible that an increase in blood-pressure might have an effect upon brain function. I asked Slaughter to determine whether the two were of the same length. The average duration of the two was about the same. He did not count the number of coincidences between troughs and crests of the two curves as should have been done. Other workers tested the possibility of relating other mental operations to these rhythms. Wright found some tendency to a connection between the rate of reaction and the position on the wave; Stevens found a variation in the estimates of time with the part of the wave involved.

While all of these results seemed significant, later work by certain of Spearman's students and by Griffitts and Miss Gordon here did not altogether confirm them. The coincidences between the two curves are sufficient to be suggestive; they are not exact enough to be entirely convincing. Since the cause of the Traube-Hering waves is not itself known, it matters little whether the attention waves can be referred to them. When two responses, each of unknown cause, are referred to each other, one is little ahead in the explanation of either.

In the same general period I attempted to relate the attention wave to the fatigue problem. The suggestion that led to the trial came from the assumption that if a wave of effectiveness was represented by the appreciation of a faint stimulus, any decrease in effectiveness would be accompanied by a briefer period of appearance. In 1903, when I had volunteered to take a group of medical students on every afternoon of the week, in addition to the usual heavy schedule, I thought a good opportunity was given to make a fatigue test. That semester I had a teaching schedule of forty-two hours a week. I made records of the appearance and disappearance of a Masson disk, early in the morning, at noon, and in the evening. Later that year, while at Würzburg, I took a few records from Külpe. My records showed a progressive decrease throughout the day. Külpe, on the contrary, showed a progressive improvement. The records were too few to be significant, but were interpreted as an indication of the difference between the morning and the evening workers. I am certainly of the morning type and Külpe thought himself to be of the evening type.

Other attempts made to determine if there might be a correlation between the attention waves and the amount of work done were made at later times. The first was lost through an unfortunate circumstance. I was revising a translation of Külpe's *Introduction to Philosophy* that I had made with Titchener while still at Cornell. In the revision, I used typewriting the translation as material for mental work. I wrote straight ahead and indicated the time on the manuscript each quarter-hour. Between times, at intervals, interruptions were made to take records of attention waves. The intention was to see what the output amounted to for different periods of work, when doing work under the normal incentives. Professor Titchener was to revise it and that would furnish a measure of the accuracy as well as of the rate. Unfortunately, the manuscript was sent to the English publisher just before it went bankrupt, and by the time the firm had been reorganized twice the manuscript was lost and the check was impossible. Ten years later I repeated the experiment by writing a book that did not require much collection of new data and keeping a record of the time as I wrote. In this instance the typewriter was provided with an electric contact that would record the end of each line and respiration was recorded during the period of actual writing. At the beginning and the end a record was made of blood-pressure and of the attention waves. Other tests of steadiness were made that there might be as complete a picture as possible of the mental and physical changes during two hours of continuous hard work. The preliminary results were published in a preliminary form in the *Proceedings of the Eighth International Congress* at Groningen. The final results have not yet been published.

The first book published was the *L'Attention* in French. The book was written before I left for a trip abroad in 1903. It grew out of the work on the thesis, and of the experimental work on the attention waves. It was primarily a summary and interpretation of the work already done on attention. The material collected outgrew its title and extended the principles involved in attention to many fields that lay outside the title as narrowly interpreted. The book was published in French because no American publisher could see his way to publish even so slightly technical a work as that. Meantime, Titchener knew I had the book ready and when Vaschide sounded him about a book on attention for the *Bibliothèque internationale de Psychologie,* he suggested that he get in touch with me. I sent on the manuscript and it was accepted. The time required for translation was long so that the work finally appeared in 1906. It was enlarged for an English edition in 1908.

The general purpose of the book was to trace the phenomena of selection which we ordinarily call attention to their specific occasions or causes. The attitude was entirely empirical. The first chapter was devoted to a description on the basis of earlier writers of the concrete changes in consciousness which are designated as attention. Increase in the clearness of one group of ideas or sensations was made the primary characteristic of attention. Consideration was given to the notion that this was identical with a change in intensity, but decision as to the identity of the two was left open. Analysis and synthesis were made subordinate to changes in clearness and regarded as dependent upon them.

Careful examination was made of the theories that would refer attention to motor processes in the organism, such as Ribot's, and the general conclusion was reached that the motor processes were not true causes but, at the most, were, with the mental changes, common results of deeper lying antecedent changes. The most important contribution lay in seeking the antecedent processes that could be regarded as the real conditions of attending. In general, the classification was followed that had been given to the contributions to the reading processes in the dissertation. Conditions of attention were divided into objective and subjective. The objective consisted in the characteristics of the stimulus which made it likely to enter consciousness, the intensity, duration, and extent of the stimulus. The subjective were constituted by the immediately present, and the more or less remotely past events in consciousness. They were enumerated as the idea in mind, the mood of the moment, wider educational factors, heredity, and the more general instinctive factors. The mood of the moment, which was intended to designate the general perceptive attitude, lost its intended significance when it was translated into French as *mode*. The essential point in the whole interpretation was the insistence that attending was an expression of definitely empirical factors which could be analyzed experimentally and that explanation could be given without reference to any force or faculty.

Before the English edition was prepared, Külpe and his students had done their work on the influence of *Aufgabe* upon both perception and association. These I regarded as merely a more active form of what I had called "mood." In Külpe's experiments it was demonstrated that when an observer was asked to look for one object or for one aspect of an object he would see that in preference to all else. In the English edition the mood of the moment of the first edition was replaced by three separate phases: the question in mind, which arises spontaneously to the observer; the task or problem set by another as in a question; and the attitude, a more general tendency to appreciate things of a particular kind. The last was most closely related to the old mood.

In both editions great emphasis was placed upon the importance of the concrete as opposed to the abstract. This came out especially in showing that the two words, interest and effort, used popularly to designate the causes of attention, were not separate entities or forces but were merely names to designate groups of conditions and the conscious states that accompanied the different types of attention. Interest designates the pleasure that accompanies attention due to attitude, education, and the inherited characteristics of the individual. It mistakes the effects of attending for the cause, for the members of the class are most universally accompanied by pleasure, and this is obvious, while the antecedent conditions receive little notice. Similarly, attention ascribed to effort or will is really determined by social pressure, which drives the man to do what society approves rather than to what pleases. Attention of this class is always accompanied by diffuse strain sensations. These we call effort and regard as the cause of attending in spite of the fact that they follow rather than precede the clearing up of the mental state. Both are due to antecedent conditions of a concrete type, but they are characterized for the individual rather by their accompaniments than by their real antecedents or causes.

In both volumes the application of the notion that mental states were determined by events which had preceded them in consciousness was extended from receiving sensations or attention proper to the control of the reappearance of experiences, the recall through association. It was asserted that the order of presentation of old ideas was determined in part by the connections between ideas formed by the order in which they had originally been presented, but that these tendencies to recall were finally controlled by the same series of more subjective conditions, active in attention. Particularly, the purpose and the mental attitude are effective. This was suggested by the work of the thesis. There the effects of calling an associated word before showing the printed word proved itself sufficient to decide what word would be read when the word on the screen was much mutilated.

The French edition was written and not revised before I knew of

Watt's work on the effect of *Aufgabe* on association. I was at Würzburg while he was working on the problem, but did not act as subject for him, nor know specifically what he was doing. The book had been finished before that time and was sent off without further revision. A complete revision was made before the English edition was published and in that Watt's work was mentioned and his results definitely incorporated in my treatment.

The principles laid down in these earlier chapters were elaborated in the treatment of the more usually mentioned psychological processes. In the French edition, a chapter was devoted to attention in memory, will, and reasoning. In the English edition, the treatment was expanded to devote a chapter to each of the topics. New chapters were added that showed how attention was related to feeling and to the self. The former was devoted to showing the close relation between theories of feeling and attention or apperception. The chapter on the self was a reprint of a presidential address. It was appropriately included in a book on attention since it showed that self, as the term was used in philosophy and popularly, was largely involved in control of thought and action. The real control in each of these cases was to be explained by the interaction upon each other of all the various experiences of the individual. This interaction was also the determining factor in deciding what should be attended to. The self as I thought of it is as concrete as any other experience. It is a convenient term for the whole man active, and so overlaps very considerably upon the field designated attention.

The concreteness of the explanation was furthered by the attempt to relate attending to the processes of inhibition and reinforcement in the nervous system. In this the work of Exner and the early investigations and interpretations of Sherrington were followed. Both had shown that inhibition of one act by the activity of another portion of the brain was possible and both had assumed that facilitation of the action of one part of the brain might follow upon the action of another portion. It was shown that one could readily translate the conditions of attention as I had outlined them into mutual interaction of different parts of the cortex. The idea in mind would correspond to a continuance of the activity of the part affected so that it would be more readily aroused by a sensory stimulus of approximately the same character as the preceding one. Attitude or purpose would, on the nervous side, have as condition the partial activity of a group of neurons induced by the preceding stimuli or earlier activity. This would be more obviously the case where a preliminary request had been made. Education and the more remote conditions would prepare the way for attitudes by grouping the nervous elements into a complex that would be excited by the incoming stimulus or idea as a unit. All of the conditions are to be looked upon as dependent directly upon neurological factors, even if the evidence for the particular nerve processes was of a general character, sometimes, at least, bordering upon speculation.

The main virtue of the approach to attention utilized in the book lay in the attempt to give an empirical explanation of each of the phases of the phenomena. Where data were available, actual experimental evidence was given for each point. Where this was lacking, observations from everyday experience was used and care was taken to emphasize the source of the evidence. Even where the specific data were lacking, the method at least avoided using the faculty method of explanation. Attention was never regarded as a specific force, but was always looked upon as the expression of earlier experiences. Keeping the possibility of nervous processes of explanation in mind also contributed to the same end. In one way it also avoided the atomism of association that has recently been condemned and avoided in another way by the Gestalt movement. I thought of the wider organization as working through and upon the associations and controlling them rather than of replacing the associations altogether by the organizations, as the Gestalt theory would.

The second large piece of theoretical work I began concerned itself with reasoning. Interest in reasoning began with a course on the modern logicians, especially Bradley and Bosanquet, with Professor Creighton at Cornell. On rereading my dissertation for this autobiography I found that I had appended to it a note that I was at work on an application of the principles discussed in it to the reasoning process. This is the only indication that the problem was in mind, as the first articles that bore on the subject did not appear for several years. The first paper that could be regarded as connected with the subject was read at the meeting of the Psychological Association in Chicago and later published in the *Philosophical Review* for July, 1904, under the title "The Psychological Nature of Causality." It was in essence an attack on the Hume theory that cause and effect could be regarded as merely one of the laws of association and that mere frequency of succession of two events could be regarded as constituting the criterion for regarding one as the cause of the other.

As opposed to, or rather, in addition to, mere successive occurrence, I insisted that there was on the phenomenal side an ascription of force to one and of passivity to the other. This is represented in the ascription of strain sensations to the active agent, similar to those that one feels in one's muscles when active. When two events are thought of as merely succeeding each other in time, both are regarded as passive, no effort is assigned to either, by what Lipps has called empathy. When regarded as causal one is given the strain sensations. More important is the question as to what leads an individual to assign the strain to one and regard it as active. This is only in very small part mere frequency of succession. More depends upon the general probability of adequacy to the effect, a probability that is estimated in the light of general experience with forces of the same type. If the cause seems adequate to the effect it is likely to be accepted with few repetitions, if it is inadequate a very large number of repetitions will not bring conviction. It was pointed out in the article that telepathy would not be accepted even with 90 per cent successes for there is no adequate mechanism for projection. Ten per cent of failures with a radio receiving apparatus would be accepted as due to chance mechanical defects, for there the mechanism is known to be present and effective. De Rostand made his cock assume that he caused the rising of the sun by crowing, but this would not harmonize with the general experience of mankind. Mere frequency of occurrence does not give conviction of causation.

A more complete discussion of reasoning than had been given in the *Attention* was offered in the *Psychology of Reasoning,* published in 1910. The incentive to write the book was furnished by an invitation to give a series of lectures at Columbia the first semester of 1908-1909, while substituting for Professor Cattell. There were eight lectures in the series and they were worked over into a book of ten chapters. The position assumed was that reasoning is a process that goes on in the concrete human consciousness and that, on one side, it follows the same laws as recall or perception. As distinct from logic, which is interested only in knowing what is true, psychology studies the actual process by which conclusions are reached by a particular individual and how he knows that they are true when once they are attained.

A question that presents itself is the relation to the treatment given by Dewey. Before I wrote, Dewey had published his *Studies in Logical Theory,* which I knew. The more popular and comprehensive *How We Think* was developed at about the same time as mine. They were alike in assuming that thinking is always a function that is performed only when there is some definite occasion. One never thinks unless one must. The divisions of the concrete process followed much the same lines, although they were not identical. I used the names that were given to processes in the older logic, although they were changed in their applications. Like Dewey, I insisted that reasoning always started when some purpose was thwarted. This thwarting might be an actual physical process or it might be in the way of developing a clear organization of thought. The process of removing the difficulty always takes the same course, whether the obstacle be physical or to thought alone. The first step is to understand the obstacle, which I called judgment, for it is really taken when the new can be referred to an old concept or something else that is familiar. The next step is to find some way of removing the difficulty. This, the really essential part of the reasoning operation, I called inference. Then the various suggestions that come in the process of removing the difficulty must be accepted or rejected, and when one seems to be valid, it must be justified to the thinker or to his hearers. This process is called proof. These four operations are not always sharply marked off, as they are short circuited at times and at times certain ones seem not to be necessary. On the whole, they are convenient points of reference and can be regarded as constituting a complete act of thought.

Back of the processes there are certain general functions which have always been regarded as characteristic of reasoning and are used in one form or other in almost every act of thinking. The first of these is meaning. It may be asserted that, while meaning is often regarded as subsidiary to mental content, it is really the first function of all mental operations and may be regarded as the real datum for psychology. One knows that one means and what one means long before he knows anything about the structural components of mental processes that are theoretically regarded as making the meaning possible. While meaning is antecedent to all else in the appreciation of the thinker, it needs to be explained in terms of the sensory components which are more obvious to the analytically minded psychologist. There is always a definite content in mind when one means, and this content may be regarded as an element in the instrument of meaning.

We can study the nature of meaning most readily in its development. The meaning develops by means of the different connections in which a word or object is used. After it has presented itself several times in connection with another object or occasion, it comes to represent the object or occasion. At first it represents it by actually recalling it. Later it alone may come and the other will be taken for granted on its reappearance. This is the case when a word has, by frequent use, been made to mean an object. When it represents a class the process is the same save that any one of the group may be represented by the first idea. As opposed to Bradley and Bosanquet, meaning was given an empirical psychological explanation and was not left as a member of a hypothetical realm of pure thought. It was not assumed to be a specific mental content as Titchener seemed to assume in his *Thought Processes,* published at about the same time. It was also assumed that meaning gains in fullness from the more remote elements of experience, that the references constituted a constellation rather than a suggestion of one element alone. The more one knows about a subject, the wider the meaning. Thought is always meaning as opposed to sensation, but the meaning is dependent upon ideational processes and also upon the action of the nervous system.

The view suggested in this book and later was that meaning and thought were synonymous. As opposed to Titchener, who found the meaning in a second idea or sensation, I held that the reference comes through the partial opening of association paths, paths which, when they open fully, recall the object that is merely meant when the partial opening occurs. The tendency to open gives rise to a specific awareness, which serves in place of the specific recall. This anticipatory awareness performs all of the functions that would be performed by complete recall. I suggested that it was the nervous correlate of what Woodworth and the Würzburg School called the pure thought process.

Closely connected with meaning is the concept. In my view the concept was subordinate to the meaning. Any mental process that had a general meaning was a concept. What the nature of the concept might be was relatively unimportant. What it meant was essential. If the meaning was of a group of specific members of the class it constitutes the class concept as interpreted by Galton; if the meaning applies to qualities, we have the concept with an implication of intention. The content might be the same in both cases. A form of concept which affects the content more than the meaning is provided by those cases in which the content of an experience is changed the better to conform to the separate experiences. In many instances of perception one sees an object as it has been transformed under the influence of numerous tests. Thus we always see square objects as square whether they fall upon the retina so as to give right-angle outlines or not. We substitute the angle that we know they must actually have for the angle which is thrown upon the retina. This resultant I called the type, and pointed out that many actual percepts are really types in this sense of the word. This was hinted at in the *Reasoning* and was developed in more detail in an article, "The Rôle of the Type in Simple Mental Processes" (*Phil. Rev.,* 1911, 20, 498-512).

A third function peculiar to thought that received special treatment was belief. Belief is important in the reasoning operation since it serves to give temporary confirmation of the truth or adequacy of each step. Like meaning, this is purely a functional characteristic of mental life. We know that we believe, but it has no corresponding mental content. With Bain it was insisted that belief is rather negative than positive. Disbelief can be analyzed into conflict between a statement suggested by someone else and the knowledge of the individual, whether that knowledge at the moment be explicitly or implicitly conscious. When the sum of the knowledge is definitely opposed to the statement or to a conclusion which the thinker himself has reached, it is rejected at once. More characteristic is the state that results when there is a balance between considerations that favor and those which oppose the statement. This gives rise to varied strain sensations and a constant alternation of opinion between acceptance and rejection. It is the state that we know as doubt and is highly unpleasant. When one makes a decision by permitting one series of data to dominate, or when a new point of view comes that makes one acceptable rather than the other, the strains vanish, the whole mood is quiescent. This is what we call belief, or, if put in the negative way, disbelief. In general, belief is the pleasant quiescent state which corresponds to the harmony of a decision or statement with the entire mass of knowledge.

Of the active processes, the first is judgment, which was used to designate the appreciation of the preliminary situation. This use was based first on the assumption that the proposition of formal logic was identical with judgment. Assigning this a definite place in the concrete mental operations offered some difficulty. The nearest approach to that that had been made was by Bradley and Bosanquet, who defined it as the process of assigning meaning to the given. They also point out that the subject is the new or unknown, the predicate the meaning which amplifies or explains the situation. In ordinary speech the sentence is a designation of what is perceived to belong under the concept or general notion. The subject represents the name or the quality just previously applied to the object, while the predicate is the characteristic that is important at the moment and so represents the real act of judging. In the concrete thought operation, this is applied in understanding the difficulty which spurs to reasoning. When the car one is driving suddenly stops and examination of the tank shows it to contain no gasoline, the single word "gone" brings to the companion a full appreciation of the situation.

As an effective step in the thinking process, judgment prepares the way for action by determining exactly what is wrong. The mechanism is approximately the process of perception. The difference lies in the fact that the appreciation is made explicit by reference to organized earlier experience or concepts. It was emphasized that various forms of appreciation are involved in thinking. One may be interested in determining which of two objects is longer, which gives the judgment of comparison; or, in making an absolute estimate of value, the judgment of evaluation. The processes are the same psychologically. The attitude growing out of the question or the problem determines what judgment is passed. This again determines what concept is attached. The formulation of the judgment in language, the proposition or the sentence, depends upon many circumstances incidental to communication. This may affect the social situation but has no bearing upon the mental operation, and is only in part determined in character by the nature of that operation. One needs to recognize that there are these different ways of formulating the results of the judgment, and this made necessary the distinction between the different formulations.

Probably the most original part of the theory of reasoning lay in separating the process of obtaining the solution of the problem from the proof that it was adequate, and in showing that the older logician had confused them. The syllogism was always regarded as a process of reaching conclusions, and Mill assumes the same function for induction, or for his still simpler process of reasoning, from a single instance, and that a particular rather than a general. I pointed out that there were really two distinct functions, the process of finding a solution for the problem that had been set and understood in the judgment, and, finally, the operation of testing or proving that the solution was distinct. The syllogism is a process of proving only, and could never be used as a means of discovery. Nothing in the syllogism gives or could give any direction to the thought process. Actual observation of the instances even of theoretical thinking shows that the conclusion always comes before the statement of the universal principle that is called the major premise. Induction also would have no specific value if it began with a blind accumulation of instances. In both cases one has a preliminary hypothesis which is believed to be true and which is tested either by the deductive method of the syllogism or by induction, whether by observation or experiment or both. The formal logician has apparently given all of his attention to the proof process because that is the one which is socially effective and so is most obvious. It can also be taught. The process of developing the hypothesis is completely subjective, and is so little subject to rule that it ordinarily escapes observation. The logician assumed that he should be able to say something of the active thinking process, and, having developed the process of proof, which is closely related to the other, he apparently merely took it for granted, without further thought, that the two were identical.

I called the process of reaching the conclusion inference. The use of the term is not without objection, but the application made seems in harmony with the derivation of the word and with current usage. The method of reaching a conclusion does not follow any regular course. So irregular is it that the term trial and error was applied. When a problem is to be solved, one suggestion after another comes, and the attitude of the thinker seems to be one of waiting for a satisfactory idea to present itself. The act seems to be as little controlled as are the movements made by Thorndike's cats in obtaining release from a puzzle box. This suggested the trial-and-error notion. Of course, each separate suggestion springs from the stimulus of the situation and is determined by the previous connections under the influence of the attitude of the thinker. But there are so many preformed connections and the attitude changes so frequently that the suggestions cannot be predicted in advance. Also, no rules can be given that will hasten the appearance of the right solution. One can only wait and watch for the answer to present itself. There is little control.

The fact that one cannot control the appearance of the suggestions for solution does not mean that they are not subject to law. We may assume that they depend upon the laws of association under the influence of context, as do other recall processes. The thinker cannot voluntarily discover the cues that will bring the solution at the suitable occasion. He seldom attains the end with a single suggestion, and often he must delay hours or days before an acceptable suggestion will make its appearance. Nearly always there are a number of ideas which succeed one another while one revolves the problem, and each is rejected in turn. Wallas, in his very interesting *Art of Thought,* began by taking exception to my interpretation of the situation, which he thought held out little hope of a solution, and devoted himself to a full analysis of the conditions under which satisfactory solutions appear. His final result, however, was not very different from mine. He found that one should not know too much, that much was to be gained from giving up and taking a walk or other forms of diversion, but the amount of actual control which he demonstrated is relatively slight.

Assuming that ideas come more or less at random, so far as the intention of the thinker is concerned, it is obvious that the process of selection is a highly important factor in the thinking process. The immediate determinant of the selection is the belief process. Each claimant to be a solution is tagged as true, possibly true, or false, or there is an alternation between acceptance and rejection or doubt. Many of the suggestions are rejected at once, because it is obvious that they do not fulfil the conditions or are not in harmony with the experience of the individual on that point. Some are accepted tentatively and given a more definite and explicit test later. Others are accepted immediately. Belief is the real determinant of reasoning. As was said above, belief is the result of a reaction between the past experience and the present environment of the individual. The nervous correlates can only be speculated about, but any of the theories of irradiation from parts of the cortex not fully active would give a possible explanation.

Even after a solution has been accepted as true, it is essential that the retention be justified. This is done through proof, and proof is essentially nothing more than making explicit reference to the earlier experiences that are acting implicitly in determining belief. The function of the syllogism is really in connecting the conclusion that has been derived through association processes with the universal principle that is contained in the major premise. That means that the order of the syllogism does not at all correspond to the order of actual thought, in fact, exactly reverses it. The function of the syllogism seems to consist in stating in definite terms the principle that crystallizes the knowledge which implicitly controlled belief. The statement adds no knowledge. Its only function is to make obvious what previously was latent.

On the whole, this volume on *Reasoning* attempted to show that thinking was the development of the ordinary mental processes. It made more of the notion of concept and meaning than in other parts of psychology. The concept is the formulated results of earlier experiences. They fix the interpretations and serve as points of reference in the simpler interpretations. The general proposition is the analogue of the concept on the more complicated level, and also acts in the same way as point of reference and justification for the results of inference. These are the norms and exercise a controlling function. The active course of reasoning is determined by desires and by the necessity for the elimination of conflicts and inconsistencies. In this it is an expression of the growth of knowledge in man and in society, and of the practical and intellectual needs of the individual and of the group.

One incident of this middle period may be of general interest. Sometime between 1910 and 1912 Watson was showing me through the laboratory at Johns Hopkins. I was very much struck by the apparatus he was developing to investigate the color vision of animals by the use of spectral lights. Apparently with too-well concealed enthusiasm, I remarked, "So this is psychology," counting upon the emphasis or attitude to make known my appreciation. While in Germany a little later, I read his article in the *Psychological Review* of 1913, in which he first stated his radical behaviorism, and was very much surprised at his interpretation. It will be recalled that he said that a psychologist to whom he had been showing his spectral apparatus had remarked, "So this is psychology," and that this was interpreted to imply contempt for anything that was not introspection. The incident might be interpreted as evidence that even the prince of behaviorists cannot correctly interpret emotion from behavior alone, even when the behavior involves words. It was so long after the publication of the article before I saw it that I neglected to correct the impression. I may take this belated occasion to remove a possible stigma from the camp of those who do not accept behaviorism in all of its tenets.

In 1912, I published an elementary text, *The Essentials of Psychology.* As a useful text must be, it was a compilation of the more important facts of psychology. It embodied in abbreviated scope the main outlines of the material previously published in my own monographs, and was extended to include the whole field as it appeared at the time. It retained the point of view of the *Attention,* but extended it to include the nervous system, sensation, and the details of space perception. It attempted to coordinate the materials of the psychology of the day from a purely empirical standpoint and with as little reference to schools as possible. Anything in the way of a contribution was limited to methods of presentation and organization.

This was followed in 1916 by a larger book, *The Fundamentals of Psychology,* also intended primarily as a text. It gave room for a greater amount of detail in the presentation of experimental data, and could presume a greater maturity on the part of the students in the appreciation of advanced theory. The division of the texts grew out of local needs. Almost from the beginning at Michigan we had had two courses, a longer, which extended through the year and might be accompanied by laboratory work to give a thorough survey of the field, and a briefer, introduced first at the request of the men in Education, which lasted but a semester.

During the War, work was very heavy because of the taking over of courses that had been left by members of the staff who had gone into national work of different kinds. This left little time for independent work. During this period I was interested, as was every one else, in the social phenomena presented. The result of this interest was *The Psychology of Nationality and Internationalism,* 1919. The volume was suggested by observation on the mixed allegiance of the Greeks who had returned to their native land and whom I met in a trip to Greece in 1912-1913, during the Balkan-Turkish war. The War itself suggested the possible shift to internationalism. This offered an opportunity for a more extended treatment of instinct than I had given in the texts. There I began to insist upon the affective values which act as selecting agents as the part of the entire instinct which is probably native. It was pointed out that much of what passed for instinct in the texts was probably learned, but that the pleasure that attached to certain results, when attained, constituted the selecting agent in the chance movements. What is inherited is a tendency to continue acting until an unpleasant stimulus is removed or a pleasant one obtained, and, more important, a determination of what shall be pleasant or unpleasant to a given species. This point of view was extended in an article published in the Washburn Memorial Volume in 1929 to make the pleasure itself largely dependent upon a tendency to have one movement or group of movements persist in response to stimuli of any kind and to have all movements cease under stimulations of another kind. That reduces the inheritance of pleasure to one of the inheritance of a tendency to a specific response.

Another point of psychological interest in this book was the objection raised to thinking of a community mind as a real entity. A group has no consciousness apart from the consciousness of the separate individuals who compose it. The group does show certain tendencies to respond that are different from those shown by the separate individuals who compose it. This can be explained in terms of the influence exerted by each upon the others. What may be called the social instincts must be made to bear the burden of the explanation of these reactions. Fear of the mass, on the one hand, which becomes strong enough to show itself in paralysis or in an incoordination of movements of all kinds, is the more important of these. In a weakened form, this becomes Cooley's social pressure. On the other hand is the increased strength of response and general adequacy that may show itself in a congenial group, and a confidence in which each takes courage from the others. That a group or the presence of others will, on certain occasions, decrease and on other occasions increase the efficiency leaves the final result uncertain. Nevertheless, both conditions may be observed and both are necessary to explain the social phenomena. Social phenomena must be analyzed into their component parts and not explained by mere reference to a group mind.

Of the forces which hold a group together it was asserted that a common hate was the most effective. That hatred of a group of surrounding nations increased the solidarity and so the strength of that nation, and the greater the hate the greater the unity. In a single community, dislike of an individual or of a group would develop unity in other groups more surely than would common likes or community of action towards a desirable end. Political or even religious groups need recognized evil or a rival that can be hated to be really effective. The same theory finds numerous applications in any type of society. It should be noted that Martin in his *Behavior of Crowds* developed the same theory from another point of view, and on mainly pathological evidence, at about the same time. There is no evidence from the dates of publication that either could have borrowed from the other. I did not know of his treatment until some years after my book was published. Both seem to have been an outgrowth of observations of war propaganda.

An interesting illustration of the effectiveness of the hate propaganda, or possibly of its wide appeal, came a few years later. In 1921-1922, I spent a year in France and gave a series of lectures at a number of universities. One of the series was an adaptation of this chapter on "Hate as a Social Force." I had given it at several institutions without arousing other than favorable comment, and chanced to repeat it at Grenoble the week the French entered the Ruhr to enforce the demands for reparations. I had made no reference to French politics or to reparations. In fact, the illustrations chosen were all drawn from the War and were perfectly good pro-ally propaganda. In spite of all, when I finished, the rector of the university, who had been exceptionally gracious in his introduction, rose as I finished and denounced me in no uncertain terms for interfering in French politics and giving comfort to the enemy. I had only asserted in general terms that if one arouses the hatred of a nation, that nation will be strengthened in its unity and ultimately in its effectiveness. My French was too feeble to attempt a rejoinder. The lectures had been translated for me and I did not venture on an extemporaneous explanation. My embarrassment was great, but I did not fail to note the amusing side.

A final chapter outlined the similarities between the national and the international consciousness and attempted to trace the limits within which a society of nations might utilize the forces of nationality in gaining strength when once it had been established. This would apply whether the society were definitely organized or were a result of an unorganized good will. That offered little that was new psychologically. It merely showed how the social laws might be extended to the wider field.

The year in France in 1921-1922 was spent in learning a little more French, in preparing and giving a series of lectures, and in becoming acquainted with the smaller French universities. One quarter was spent at Toulouse, a second at Montpelier, and the third in Paris. I prepared four lectures for the general tour, of which two were published. One showed the many points at which the trial-and-error process finds application in the more truly mental operations. It first found its application by Thorndike in modern times, in explaining the motor learning of animals. I had shown that it was the main factor in the reasoning process, as well as in recall through memory. In this article I showed that the process very probably is an element in the development of percepts. A percept does not correspond to the sensations that are aroused in the sense organ or in the brain through the sense organ, but are always more completely organized and consistent. It was asserted that this consistency is made possible by a series of trials which finally develop a notion that will agree with various groups of sensations by eliminating certain ones of them. Those eliminated are overlooked and forgotten, and what remains is regarded as the real object. Concepts are merely a further elaboration of the percepts and arise in the same way. They eliminate still more of the unessential or inharmonious factors and leave what agrees with all of the others.

Another lecture was a summary of my theory of reasoning, brought up to date, and with the more outstanding parts emphasized. Most was made of the point that the active parts of reasoning and the confirmation or proof were distinct and that the syllogism was effective only in the proof. It was not a process of thinking in the sense of reaching conclusions, or solving problems. When I reached Paris, I found that I was announced for a special course at the Sorbonne. For this I wrote a series of three lectures on the development of psychology in America. The first part on the American schools was published in the *Journal de Psychologie* in the following year. While in Paris I made a few experiments on fatigue in confirmation of some work that had been done with Griffitts before I left America. Professor Piéron kindly offered the facilities of his laboratory and I availed myself of it so far as time permitted. The article was published in the *Proceedings of the International Congress of Psychology* at Oxford in 1923.

While in France, and before and after, I devoted some time to the writing of *Education as the Psychologist Sees It,* intended to be a popular presentation of what psychology has to contribute to education. Possibly the central idea was developed in an article in *Popular Science Monthly,* published in 1921. In this I pointed out that many of the advantages which we take credit for as a result of education in our schools and universities are really a result of selection. That only those who have the greatest ability naturally, at least those who stand in the upper 10 per cent, succeed in passing through our entire educational system. These men would be the successful men, whether educated or not. The universities assume that what they do to them makes them what they are, while the chances are that nothing that can be done to them would spoil them. The only question is whether anything done to them by education really improves them over what they would be without that influence. On this we have no definite evidence.

I attempted to bring together what few facts we have that bear on the point of the effects of training. I also presented what seemed to me the more important contributions of psychology to each of the different problems of education. These chapters contained a maximum of psychology and a minimum of strictly educational application. It was written on the assumption that a brief summary of the more important applications of psychology to learning and related processes might be useful. The assumption seemed justified by its success.

A book that was developed for a special purpose was the *Psychology of Language*. This was an outgrowth of a request that Professor Meader, of the Department of General Linguistics, had made that I cooperate with him and Professor Scott, at that time Head of the Department of Rhetoric, in a course on the psychology of language. This course was given for ten years or more. Finally, it was suggested that the lectures be put into book form, which was done. It was intended to bring together the phases of psychology which bear upon language in many of its aspects. In addition, Professor Meader developed some of the features of language changes and similar, more strictly philological problems which could be related to the psychological laws.

So far as anything more than the elements of psychology were involved, I tried to emphasize the great dependence of the separate parts of the language units upon the whole situation in which they developed. That eliminates all theories of the sentence that would reduce it to a form of equation, or that would make the symbols have value apart from the context in which they are found. If one goes back to the theory of the sentence, subject and predicate must be regarded as constituting different aspects of a common object or situation that attract attention successively. The successive awareness is what holds them together. They are not equal to each other; if they were, they would be identical and the statement a tautology. That means also that they are united by a common attitude, and this attitude or purpose must be present also in the mind of the hearer if they are to represent to him what was present in the speaker. While this was but one of the problems considered in the book, it illustrates the emphasis that was given to the particular situations in the explanation of each topic. The general thesis was that language is a psychological process and each step must be considered in the concrete relations if it is to be correctly understood.

In the winter of 1927-1928, I was asked by a representative of Mr. Norton if I would write a history of psychology. As I had given a course in the field for a number of years and had no plans for further work after the *Language* which was then in the hands of the printer, I consented. I also did not know that so many others were already engaged in the task. I had heard the summer before that Murphy was working at one, but, as nothing definite had been said about its publication, I thought it might be long delayed. I agreed to have it finished by January, 1929. My daughter was studying in Germany that year and was to have the summer semester in Berlin, so I went over there for the summer to work on it. I desired to know more of the Gestalt movement, also, and the chance to hear of it at first hand added an incentive. I had finished a first draft of the earlier periods before going over, and had the use of the library of the Psychological Institute and of the University for the later periods. The Institute library was very convenient and complete for more recent times. The University and State libraries could not be said to be convenient, with the restrictions of permitting books to go out only between twelve and one and then on slips left the day before, but they did have most of the books I needed. Between them, I could obtain most of the sources I required. Much could be accomplished in the two months' time free from regular duties.

The original intention was to write a brief sketch of the earlier periods, giving in each epoch only the contributions which really influenced later writers, and then to give a fuller account of the more recent men who could be said to have developed the modern psychology. As time was short and considerable difficulty appeared in extending a treatment beyond the essentials without becoming verbose and repetitious, the expansion even in the later parts was not very great. I assumed it was better to restrict what was said to what could be remembered as important rather than to enter into the variations in the treatment of a subject by different men. That offers much room for expansion, and may be useful for the specialist, but the specialist will naturally go to the sources and not limit himself to a history. Where so many men are to be covered, only their most important contributions can be touched upon. Fortunately, the men who have written at greater length on these portions have supplied any lack I may have left.

I also chose to treat a man mainly for what proved to be his main influence on the course of later thought, and to neglect those phases of his work in which he mainly improved upon the work of the men who had first written in the particular field. This is very often unfair to some of the later men whose extensions may well be more important than the first statement of the man who pre-empted the problem. Sometimes the very effectiveness in statement or persistence in repetition of one of the later men has led others to forget that he was not the originator of the particular point of view. One recent case is very striking. Most, even of psychologists, credit Watson with fathering behaviorism, while, as a matter of fact, Max Meyer had propounded all of the essentials of the doctrine a year or more before Watson adopted the position.

When it came to the later periods, when psychology really became established, the amount of material was so great that I abandoned any attempt to trace the important experiments. I was forced to restrict my treatment to the more important schools and to neglect many leading men altogether. In this later period, I contented myself with outlining the position held by what seemed to me the important schools and mentioning only the men who are closely identified with those schools. This meant omitting from consideration many men who have kept clear of theoretical controversy or who have shone only as critics of schools. This list includes a number of the more important modern writers. The classification of schools is always itself open to question. I could have increased my list indefinitely, but chose to err on the side of conservatism. In discussing the schools, occasion was taken for a certain amount of criticism. This consisted in part in pointing out the historical roots from which the different schools sprang and in part in indicating what seemed to me to be fallacies or inconsistencies. This discussion was restricted to schools in order that the treatment might be as impersonal as possible.

I made relatively little attempt to summarize the results of experimental work. In recent times this has been of such great volume that it seemed hopeless. I did point out the classical experiments and many of the classical methods in each larger topic. The neglect was not at all because the results were regarded as unimportant, but because it seemed impossible to do all justice without making the book so large as to be unmanageable and to trespass upon the field of the larger texts. Any selection was bound to be prejudiced, in that it would emphasize those phases of the subject that seemed to me important to the neglect of the rest, and would be likely to give too much space to my friends and men who had worked in the same lines as myself. Again, any statement of results would be out of date in a very few years, while the general outlines may be counted on to persist at least for a longer time.

I may close with a statement of my own general position. I presume I am one of the men who would be regarded as belonging to no school. One of the more recent presidents of the American Psychological Association took me to task, by implication at least, for seeing something good in each new theory. He may have implied that this was not merely good nature. As a matter of fact, I have long believed that a general theory has little justification except as a convenient setting for facts. From its very nature, a school must be one-sided. Its statement is either a protest against an existing school or a rallying cry for a bit of propaganda. Only within limits is either useful. I have attempted to formulate a general position on several occasions, but have never pushed it very hard. In an early article I pointed out that we know functions rather than structures, directly. In an address, very brief and *ex tempore,* at the Western Psychological Association in Leland Stanford University in the summer of 1925, I elaborated the notion and showed how I would include among the functions, functions of thought in all of its forms as well as the biological functions that were given exclusive place by the traditional school of Dewey and Angell. The same position was elaborated in a paper at the Yale Congress, and in the introduction to the third edition of the *Essentials of Psychology,* published in 1930.

In all, I insisted that what one really knows directly are meanings, as was suggested in the *Psychology of Reasoning,* and that meanings are functions. This begins with the sensations and perceptions. Man knew that he was warm long before he knew that he has warm spots or warm sensations. He knew that one object was farther away than another before he knew that space existed. He knew that certain phases of objects were distinct, others indistinct, long before he knew anything of attention. Attention, itself, was first needed as an aid to understanding when man appreciated that, on occasion, this clearness was due to conditions within himself and not to the medium or even to the adjustment of the sense organ. This probably occurred even before man assigned his consciousness to structural elements as sensations. One felt pleased long before he had any question as to whether affection and sensation were different structures, or whether emotions were real states of consciousness or mere back strokes from organs of internal response.

In short, the datum of knowledge is always 'that we know' in each of its phases rather than any 'what.' We can assert that the structures, particularly the so-called mental structures, are mere artifacts that have been developed by the psychologist to make plausible a basis for the functions. Just as it was said in the paper on "The Rôle of the Type" that objects are developed by a process of trial and error to explain what is immediately known, so more truly can it be said that no one directly intuits a red as sensation structure. He has developed the concept red to explain the corresponding function, and then has found it convenient to talk of an independent red element. One is immediately aware that a red-headed boy is ten feet or more away from him, and that one is afraid he will be struck by a passing car. To explain this, one may assume that there are rays of light affecting the retina, that these arouse old memories to complete the estimation of distance, and the appearance of the rapidly moving sensations of black arouse the secretions of adrenal glands and contraction of unstriped muscle, which may explain what one calls fear. All of these are constructions, interpretations of the immediate datum that one sees the boy and the car and feels fear. These functions are real and immediate, independent of any interpretation.

This attitude towards psychological facts would not eliminate any of the explanations that are ordinarily given in present-day psychology, so far as they are adequate and true. Assume that what we know directly is always the 'awareness that' certain events are true, and we can bring in as many additional constructions as are needed to explain how we know. We can make that more immediate even than consciousness, which, in one sense at least, is an hypothesis to explain the fact of awareness. It would certainly be that if we give it any substantial character. We could also retain as many of the concepts of sensation, Gestalten, behavioristically defined attitudes, as could justify themselves on empirical evidence. The test would be the degree to which the assumption squared with observed facts and with other hypotheses that themselves were in harmony with observations.

In addition to these more subjective functions of knowing and feeling in their various forms, we would, of course, have the objectively determined biological functions that were the problems of the older functionalists and of the modern behaviorists. They would be treated in the way that they treat them, so far as that is in harmony with the facts and does not deny the functions of knowing. If accepted in this double sense, functionalism would be the broadest statement of the problems of psychology, and would take least for granted. It could use two points of view, what is seen by the onlooker and what is immediately given to the actor. It would first outline the simple functions—in fact, this has been fairly adequately done—and would then relate them to antecedent experiences by analogy to the known facts of nervous anatomy and physiology, and to as many as possible of the related functions. It would not change the modern psychology, it would, however, formulate its principles in a way to remove a large part of the dogmatism that now cumbers the work of the various schools.

LEWIS M. TERMAN

TRAILS TO PSYCHOLOGY

It is not an easy task to give an accurate account of the influences which have worked together to give direction to one's interests and to determine the nature of one's professional output. As a more or less desultory student of biography, I have been much impressed by the daring that biographical writers have sometimes shown in their attempts to psychoanalyze the characters and careers of their subjects. Although a few of the attempts in this field are interesting and more or less plausible, it must be admitted that even the well-documented life, lived in the glaring light of publicity, presents puzzling obscurities. One might suppose that, when the writer is a psychologist and is recounting his own life, the task of presenting a truthful picture would be comparatively easy. Whether such is the case is doubtful. Being a psychologist may help, and analyzing one's own life may be easier in some respects than analyzing another's, but the difficulties peculiar to autobiography may more than offset these advantages. One's memories are not only incomplete; they are also warped by systematic influences that distort the total picture. Memory of a given period of one's life is selective; what finally survives is determined partly by the nature of the events which follow. My present memories of childhood and youth may differ in important respects from what they would have been had I become a doctor or a lawyer or an engineer.

In planning a discussion of the factors that may have influenced a career, one's thoughts are likely to turn at once to the college and post-college period and to such possible determinants as teachers, classmates, textbooks, and library exposures, to the neglect of hereditary and early environmental factors that may have set the stage. This procedure, I believe, is never psychologically warranted. In some cases it is possible that the influences of the university period are primary; in other cases it is more likely that the determining factors must be sought farther back. Certainly it would be a mistake to ignore the indications of childhood experiences and preoccupations, even though heredity and environment as causal agents in molding early interests can never be clearly disentangled. That some of the experimental studies have pointed to a high degree of impermanence of interests, I am inclined to attribute to the inadequacy of the methods used. My study of biography and my work with gifted children incline me to believe that the child is indeed often father to the man. My chief anxiety about this series of autobiographies is that too few of the authors will tell me the things I should like to know about their ancestry, their childhood, and their youth. It would be hard to imagine anything more interesting than analyses of their own childhood by psychologists like James, Freud, Adler, Binet, Titchener, Spearman, Watson, or Thorndike.

HEREDITY AND EARLY ENVIRONMENT

I know of nothing in my ancestry that would have led anyone to predict for me an intellectual career. A statistical study of my forebears would have suggested rather that I was destined to spend my life on a farm or as the manager of a small business, and that my education would probably stop with high school graduation or earlier. I was born in Johnson County, Indiana, January 15, 1877, seventeen miles southeast of Indianapolis. My paternal ancestors, who for several generations had been farmers, migrated from Virginia. My father's father was born in 1794 and was of Scotch-Irish descent. After serving for a brief time as a soldier in the War of 1812, he went westward on horseback from Virginia and settled in Ohio fourteen miles south of Zanesville. This was not far from 1820. He married a young woman by the undistinguished name of Jones, said to be of Welsh descent, and by her had a family of ten children. My father, one of the youngest in the family, was twelve years old when, in 1846, my grandparents moved to Indiana and settled on a farm not far from the border of Brown County, in a region which has always been economically and culturally one of the most backward in the State. There my grandparents lived the remainder of their lives, my grandfather dying in his seventies and my grandmother in her sixties.

My father lived at home until his late teens. He attended rural schools a few months each winter for three or four years and worked on a farm during the summer. At the age of twenty he was employed by William Cutsinger, one of the most prominent farmers in the adjoining county of Johnson, and a man who was known far and wide for his positive traits of character. A year later, in 1855, he married a daughter of this farmer and shortly afterward moved to a farm in the northeast township of the same county. I was the twelfth of fourteen children born of this marriage.[1]

The Cutsingers were "Pennsylvania Dutch" who had migrated from Pennsylvania to Kentucky and thence to Indiana. They had evidently not been in America long before the westward migration, as my great grandfather spoke English only brokenly. My maternal grandmother was of French Huguenot descent and was named Deupree. Her family moved from Kentucky to Indiana at about the time the Cutsingers did. It appears, therefore, that my main lines of descent are Scotch-Irish, Welsh, German, and French.

Of my paternal grandmother's relatives I know nothing. The Termans (sometimes spelling the name as Turman or Tearman) are found widely scattered from coast to coast and are rather numerous in some of the eastern and middle-western states. The Cutsingers, with less disposition to rove, have remained for the most part in central Indiana. Previous to my own generation, I do not know of a Terman or Cutsinger who belonged to any of the professions or who had graduated from college. Those of the present generation usually go to high school and college and are finding their way into business or teaching. The Deuprees (the name sometimes written Dupré or Dupree) probably represent a rather superior strain; they had risen to some prominence before they left France at the time of the Huguenot persecutions and have done better in America than most of the other branches of my family.

Although my father was regarded by our neighbors as prosperous, the family was so large that the reputed prosperity was more illusory than real. There was no lack of the ordinary necessities, but there was little surplus for luxuries. In the household library there were probably a hundred and fifty or two hundred books, including a set of the *Encyclopaedia Britannica* which belonged to my older brother. Among the books which I was especially fond of reading were Hans Andersen's stories, *Robinson Crusoe,* a book on Arctic and African travels called *The World's Wonders,* the novels of Cooper and Dickens, the Bible, the *Encyclopaedia Britannica,* a pocket atlas, and *Peck's Bad Boy.* Unlike the authors of most autobiographies, I had never heard of Plutarch and did not care for *Pilgrim's Progress.*

I think there was nothing in my early environment that could be said to have conditioned me very specifically in favor of psychology. My reading did not, nor were my interests consciously shaped in this direction by any of the members of my family. My father read a good deal, but chiefly in newspapers, magazines, the Bible, and a few miscellaneous books. He was not very social, rarely attended church or other neighborhood gatherings, and showed little interest in persons outside the family. He was self-sacrificing, just, and infinitely kind. He was extraordinarily fond of children and was adored by them. One of his outstanding traits was an obstinate persistence in completing any task he had undertaken. My mother read somewhat less than my father, had more social interests, was less calm, and vacillated between extremes of happiness and sadness. She was devoted to her children, but did not understand them as well as my father did. An older brother and two sisters, one older and one younger, were teachers; but, as teaching was about the only avenue of escape for the youth who aspired to anything beyond farm life, the desire to teach cannot be taken to indicate any special psychological bent.

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[1] It appears from Cattell's published data in the 1921 edition of *American Men of Science* (p. 803) that my family was the only one in his group of one thousand cases with "twelve or more" children.

Doctors and ministers probably tend to have more than the usual amount of psychological interest and insight, but there was no one in either of these professions among my relatives. Gossip has been called the beginning of psychology, but I had little of the training it is said to afford. However, my brothers and sisters had a keen eye for the peculiarities of others and were accustomed to mimic and make jokes about them. To this day our family reunions are enlivened by hilariously funny reminiscences about the eccentricities of persons we knew as children. The extent to which this has always been indulged in by my family may have helped just a little to give a psychological twist to my interests.

Whatever the cause, almost as far back as I can remember I seem to have had a little more interest than the average child in the personalities of others and to have been impressed by those who differed in some respect from the common run. Among my schoolmates or acquaintances whose behavior traits especially interested me were a feebleminded boy who was still in the first reader at the age of eighteen, a backward albino boy who was pathetically devoted to his small sister, a spoiled crippled boy given to fits of temper and to stealing, a boy who was almost a "lightning calculator," and a playmate of near my own age who was an imaginative liar and later came into national prominence as an alleged swindler and multimurderer. I am inclined to think that the associations which I had with such schoolmates were among the most valuable of my childhood experiences.

The school instruction I had was very inferior, if judged by present-day standards. My elementary education was obtained in a one-room "little red schoolhouse" which could not boast a single library book. My teachers of that period were all men, not one of whom had at that time attended school beyond the eighth grade. However, I think most of them were superior in natural ability to the average elementary teacher of today. Two of them later became physicians and another a lawyer.

I entered this school three months before my sixth birthday and at the end of the first term of six months was promoted to the third grade. I "learned by heart" easily and habitually memorized most of the contents of my textbooks, including biographical footnotes in the history text and tables of populations and areas in the geography. The school day was long, there was no supplementary reading available, and the time had to be put in somehow.

I attended the same school until I was thirteen, although completing the eighth grade a year earlier. As there was no high school within reach of my home, I continued to do "post graduate" work with more advanced texts in the common branches during parts of two additional winters, but at a different school, taught by one of my brothers who had attended college. Another pupil in this last-mentioned school of thirty children was Arthur M. Banta, later to become a starred scientist in biology.[2]

Until the age of eleven, my vacations, amounting to between five and six months each year, were spent in unsupervised play, often alone but sometimes with other children of the neighborhood. The majority of my neighborhood playmates, far from providing any stimulus to intellectual development, were of the type that rarely completes the eighth grade. This, however, did not lessen my interest in them, as I admired them for their superiority in everything that called for strength, agility, or skill.

Neither from my playmates nor from my teachers or parents did I learn much about natural phenomena. The names of a few of the more common trees, plants, animals, and birds were, of course, learned incidentally, and the major processes of farm life were of necessity familiar, but I made no collections except of Indian relics, and engaged in no very systematic observations of nature.

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[2] It would be interesting to know whether any other one-room rural school in the country has contributed two men to Cattell's list.

The spring after I was eleven years old I began "making a hand" with team, plow, and wagon. Thereafter, until I was eighteen, I worked on the farm about six months each year, sometimes not losing a single week-day between April and September. As the day lasted from about five A.M. until seven or eight P.M., with only an hour off at noon, any mental development that may have occurred during this part of the year must have been due to maturation rather than to intellectual stimulation. My willingness to work was not due to any particular liking for farm life itself, nor was it primarily the result of a reasoned sense of duty. The things I had to do seemed, on the whole, pleasant; my plans to escape from the farm were motivated entirely by my desire to get an education. For the farmer boy of 1890 in Indiana, to get an education meant, first of all, that one must prepare to teach school. That step accomplished, it was possible to earn one's way through college and to enter a profession. Not to teach meant to continue forever ploughing the same fields, doing the same chores, and getting nowhere.

Preferences among school studies sometimes afford a clue to natural interests. In my own case, when I attended the rural school, almost every subject at one time or another held first place. I remember clearly that this was true of arithmetic, history, geography, physiology, and grammar. Later, in normal school and college, I preferred German, Latin, history, English, pedagogy, and psychology to mathematics or the physical sciences. When I studied botany, zoölogy, geology, physics, and chemistry, I enjoyed learning principles, and got my lessons well enough to earn good marks, but I made only the required collections of rocks and plants, had little interest in botanical classification, and never felt quite at home with laboratory apparatus. In this respect I presented a marked contrast to my boyhood classmate, Arthur M. Banta. The difference between us was clearly evident by the time we were fifteen or sixteen years of age. He was less contemplative, less bookish, and, I think, less interested in the traits of people; on the other hand, he was far more interested than I in plants, animals, and rocks. Unfortunately, no school compositions remain to tell the story of my early interests. In all my life I have never written more than two-score compositions, and of these exactly one was written before I reached the age of fifteen years.

In the school I attended and in the neighborhood where I lived there was a dearth of boys of my own age, and nearly all my playmates were three or four years my seniors. This cut me off from any possibility of becoming a leader in the social groups to which I belonged. I rarely took the initiative in deciding what should be done or how it should be done. As most of my playmates could run faster, jump farther, dive deeper, throw a ball farther, wrestle more skillfully, and swear more nonchalantly, I came to feel that I did not count very heavily in their activities. Whether for this reason or not, I developed toward older and bigger boys an attitude that ranged from respectful deference to admiration and awe. That I excelled most of them in school work as much as they excelled me in play did not fully compensate.

In such circumstances we have the soil for the development of inferiority attitudes and tendencies to introversion. My inferiority feelings, fortunately, were confined chiefly to playground situations, but my introvert tendencies still show in a very extreme score on the Bernreuter and Laird introversion-extraversion tests. In this I may not be particularly exceptional among psychologists; the introvert's tendency to become introspective and to concern himself with the motives, abilities, and personalities of others would seem to favor the development of psychological interests.

When I was nine or ten years old, a book peddler stopped at our home for the night. This one was selling a book on phrenology, the author of which I have forgotten. That evening, while we sat about the fireplace, the stranger discoursed on the science of phrenology and "felt the bumps" of each one in the family. Perhaps I remember the incident so well for the reason that when it came my turn to be examined he predicted great things of me. I think the prediction probably added a little to my self-confidence and caused me to strive for a more ambitious goal than I might otherwise have set. At any rate, I was greatly impressed and for several years thereafter was much interested in phrenology. As my older brother bought a copy of the book, I finally became familiar with its contents and believed in phrenology until I was fourteen or fifteen years old. This was my introduction to the science of individual differences and the diagnosis of personality.

By the age of ten or twelve, or perhaps earlier, it is probably not so very uncommon for a child to develop an interest in his conscious life and to engage in introspective plays. Before I had reached the teens I was often preoccupied with such phenomena as after-images, the flight of colors, and the association of ideas. My older sister and I sometimes played a kind of game which consisted in starting with a given word and calling out successive words, each associated with the word preceding, until all evident connection with the initial word had been lost. The point of the game was to get as far as possible from the starting point in the shortest possible time, but to be able to explain each transition as natural and logical.

When I was eleven or twelve years old I hit upon the fact that by the monotonous repetition of a formula I could entirely lose my sense of personal identity and my orientation in time and space. My method was to gaze fixedly at something and repeat the formula "Is this me?—Is this me?—Is this me?" or sometimes "Am I living?—Am I living?—Am I living?" until all distinction between the subjective and the objective was lost in a kind of mystical haze.

I remember with considerable clearness the following psychological experiment which I made not far from the age of eleven years. The incident is fairly definitely placed in time, as it occurred in a vegetable garden which was no longer used as a garden after I had reached the age of twelve, and the nature of the experiment was such as to favor its retention in memory beyond all ordinary events.

I was pondering over the fact that, of the countless visual impressions which one experiences, the majority vanish quickly and none ever remains so indelibly fixed in memory as to be recallable in all its details. Can it be that there is no such thing as complete and permanent memory? I decided to find out. My method was to select a small object and gaze at it so long and intently that the exact "picture" of it would *never*, so long as I lived, fade from my memory. After casting about for a suitable object, I selected a small bit of material that lay on the north side of a potato hill from which I had just forked the potatoes. It resembled, as I now recall it, a piece of semi-decayed straw or cornstalk; it was about a quarter of an inch long, had a brownish yellow color, and was shaped something like a tadpole with the tail downward. As I gazed at it, I kept repeating to myself "This time I will *never* forget—I *must* remember it—I *will* remember it if I live to be a hundred years old," and so on probably for several minutes. Even yet the image of this object seems fairly distinct and the details as I have described them fairly certain, but one who has made any study of the behavior of memory images will of course be very skeptical of the accuracy of my long-delayed report! Whether my experiment was successful or not, the reader can imagine with what interest I served as a subject, sixteen years later, in exactly the same type of experiment conducted by my classmate, Kuhlmann, in the psychological laboratory at Clark University.[3]

I trust the reader will not accuse me, because I have recounted these experiences of my early years, of trying to prove that I was a "born psychologist" and destined from childhood to preoccupation with things psychological! It is possible that such experiences are too common to have very great meaning for later development, though, conceivably, they are indicative of a predispositional personality trait that may have tipped the balance occasionally in the myriad of choices necessary to give my life the course it has had.

NORMAL COLLEGE AND TEACHING

When I was fifteen years old, my parents sent me to Central Normal College, Danville, Indiana, to prepare for teaching. I attended school there thirty weeks that school year (1892-1893) and twenty weeks the following year, returning to the farm for the intervening summer. During the winter of 1894-1895 I did my first teaching, in a rural school of the type I had attended as a child. The next year I went to Danville again and remained for forty-eight weeks, completing with the degree of B.S. what was called the "Scientific Course." The next winter (1896-1897) I taught another country school and in the spring went back to Danville for eighteen weeks to complete the so-called "Pedagogy Course," which gave me the high-sounding title of B.Pd. But two degrees were not enough, and, instead of teaching the following year, I borrowed money to remain at Danville for still another year of forty-eight weeks and complete their "Classical Course" with the degree of A.B.

This brings us to the summer of 1898, when I was twenty-one years old. In all, I had attended the Central Normal College for a hundred and sixty-four weeks, or the equivalent of about four and and a half ordinary school years. Except for a few additional correspondence courses in German and in the history of education, this was all the formal training I had, beyond the country school, until I entered the junior year at Indiana University, in the autumn of 1901. From 1898 to 1901 I was principal of a township high school

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See Kuhlmann, F. On the analysis of the memory consciousness. *Psychol. Rev.*, 1906, **13**, 316 ff.

in my home county, where I taught the entire curriculum of a four-year high school to about forty pupils.

Thus chronicled, the eleven formative years from 1892 to 1903 look pathetically barren. Actually, they were not so bad, although about as different as anything could be from the corresponding years in the youth's life of today. Judged by formal current standards, the training offered at Central Normal College may seem to have been very shoddy. With ten-week courses taught by wretchedly paid teachers whose weekly schedule of classroom instruction ranged from twenty-five to thirty hours, thorough work was out of the question. "C.N.C.," as we fondly called it, was one of several private normal schools of its type which sprang up like mushrooms in the Middle West during the seventies and eighties of the last century. These schools flourished in much their original form until the first decade of the present century. They took raw country boys fresh from the grammar school and in a few ten-week terms made them into teachers. They asked of the entering student no credentials and they lavished their degrees upon him when he departed.

Nevertheless, these schools filled a gap in the school system and served a useful purpose. In the Mississippi Valley at that time high schools were few and poor, and, because of bad roads, were inaccessible to students beyond a radius of three or four miles. Young people who could not complete a high-school course and go to the better colleges and universities would have been educationally stranded but for the opportunities such schools offered. Moreover, the instruction they gave was for the most part supremely good, despite the poor pay and overwork of the teachers. It had to be good in order to draw students by the thousand from distant parts of the country. In reality, my Danville teachers were among the ablest classroom instructors I have ever had; masters of the art of teaching. They were alert, witty, stimulating, and amazingly well informed. The four teachers from whom I learned most would probably be hard to match in any first-rate college of today. Charles A. Hargrave, who taught most of the science courses, was a gifted naturalist of the Alexander Humboldt type and, like him, largely self-taught. Gustave Spillmann, an accomplished linguist of Swiss descent, taught Latin, Greek, German, and French. The teacher of psychology, philosophy, logic, and ethics was Jonathan Rigdon, a former student of Borden P. Bowne at Boston University and a disciple of Hegel. He later took his Ph.D. degree at Boston University. The courses in pedagogy, methods, and the history of education were taught by A. J. Kinnaman, who had imbibed Herbartian doctrines from the fountain at Jena and had taken a doctorate of pedagogy at New York University. As the latter degree was not then regarded as quite respectable, he later went to Indiana University for his M.A. degree and to Clark University for his Ph.D. At Clark he did as his thesis a very meritorious research on "The Mental Life of Two Macacus Rhesus Monkeys in Captivity."

Among the textbooks which I studied under Rigdon were Sully's *Outlines of Psychology,* Dewey's *Psychology* (written in the author's early twenties), Weber's *History of Philosophy,* Bowen's *Logic,* and Bowne's *Ethics.* At about the same time I read, more or less independently, Höffding's *Philosophy,* several volumes of Herbert Spencer, James's *Principles of Psychology,* Darwin's *Origin of Species* and *Descent of Man,* Huxley's *Lectures,* Haeckel's *The Riddle of the Universe,* several pamphlets by Tyndall, Brinton's *Religions of Primitive Peoples,* Tom Paine's *Age of Reason,* and the lectures of Robert G. Ingersoll. I had to read James more or less surreptitiously, as Rigdon scorned the literary flavor of his writings and scolded us if we dared quote from them! With Kinnaman I studied Herbart (as expounded in Charles A. McMurry's *The Elements of General Method* and De Garmo's *The Essentials of Method*), Rousseau's *Émile,* Locke's and Spencer's essays on education, Quick's *Educational Reformers,* Painter's *History of Education,* Mahaffy's *Old Greek Education,* and Oscar Browning's *Educational Theories.* Represented in my browsings of this period were snatches from Aristotle, Plato, Hume, W. T. Harris, and Paul Carus. Up to the time I left Central Normal College in 1898 I barely knew of Galton, Wundt, and Hall, and, so far as I can recall, had never heard of Binet or Cattell.

It is perhaps fortunate that two of my favorite teachers, Rigdon and Kinnaman, held decidedly opposite views, Rigdon being an Hegelian and Kinnaman an Herbartian. Their clashing opinions, when one of them was quoted to the other in class, gave an atmosphere to the classroom that was tense and exciting. The more thoughtful of the students, of course, took sides. I became for the time being an enthusiastic Herbartian, perhaps because I thought I could understand Herbart and could see nothing but words in Hegel. The situation was, at any rate, more stimulating than it would have been if all my teachers had been steeped in the same philosophy. "Child

Study" was only beginning to be heard of; there were yet no textbooks on the subject, but once or twice I heard it mentioned in teachers' institutes as something very new and very significant for education. Of the psychology texts which I studied at this time I liked *Sully,* found *Dewey* obscure and rather dry, and was completely fascinated by *James,* which, unfortunately, I did not discover until my last year and could not then afford to purchase.

Danville in that day attracted a surprisingly large number of gifted young men and women. My first roommate was Logan Esarey, now Professor of American History at Indiana University and a recognized scholar in his field. The others were Arthur M. Banta (already mentioned), Oren S. Hack (now a prominent attorney in Indiana), and Elmer Thomas (now United States Senator from Oklahoma). Some of my very closest friends were Frederick N. Duncan, who later took his doctor's degree in biology and taught in various southern colleges; P. C. Emmons, now Superintendent of Schools at Mishawaka, Indiana; H. S. Simmons, now director of a teachers agency; and Bert D. Beck, who entered the ministry and became a doctor of divinity from Boston University.

Two other influences of this period deserve to be mentioned. One of these came from the Teachers' Reading Circle books which each year were adopted for state-wide use. For a given year there were two or three books which every teacher was required to purchase and study. In fact, the books were purchased for us and the cost deducted from our salaries. One Saturday each month all the teachers of a township (in my township there were nine) met at one of the schoolhouses for an all-day institute session. The greater part of the day was devoted to discussion of the Reading-Circle books, a definite assignment being made to each teacher a month in advance. Thus, during the five years of my teaching prior to entering Indiana University in 1901, I read and studied rather minutely twelve or fifteen books on education or psychology. Among them were Arnold Tompkins' *Philosophy of Teaching* (of Hegelian flavor), W. T. Harris' *Psychological Foundations* (also Hegelian), Ruskin's *Essays,* Hughes' *Dickens as an Educator,* Bryan's *Plato the Teacher,* and James's *Talks to Teachers.* The last two books interested me profoundly. Bryan's attractive presentation of Plato made me want to take up the serious study of philosophy, and *Talks to Teachers* greatly intensified my interest in the psychological aspects of education. Turning through my old copy of this book, I find that my marginal notes made at that time are especially numerous in the following chapters: The Necessity of Reactions, What the Native Reactions Are, The Laws of Habit, Memory, Apperception, and The Will. The chapter on memory has by far the most. I am inclined to think that the influence of the Reading-Circle books was real and lasting, for they helped to give me both a philosophical and a psychological interest in education. Henceforth teaching was not simply a means of earning a living or of providing funds for an additional year at college, but a profession of intrinsically absorbing interest.

The other influence of this period came from marriage and the birth of my first child. In 1899, a year after I left Central Normal College, I was married to Anna B. Minton, a teacher whom I had met at Danville three years earlier. She had begun teaching at even an earlier age than I, had come under the influence of the same teachers at Danville, and had exactly the same objectives for me as were already shaping themselves in my own mind. Less than a year later, our first child was born, bringing a new and vast psychological interest into my life. My wife reminds me (I had forgotten it) that two to three years later, while a student at Indiana University, I told her my interest in our baby had determined me to become a psychologist. Whether or not the influence was so decisive as this would indicate, it is certainly true that I was introduced to the child-study movement at a psychologically favorable time in my life.

INDIANA UNIVERSITY

In 1901 I was able to borrow the money to carry out my plans for further schooling. By going on, I hoped to prepare myself for a position that would enable me to teach psychology or pedagogy in a normal school or college; failing in this, I could fall back upon a high-school principalship or superintendency of schools. My immediate goal was the A.B. degree from a standard university, with the A.M. as a possibility. The Ph.D. seemed too remote and unattainable to plan for definitely, though I had vague dreams of reaching it sometime.

I chose Indiana University for several reasons. Kinnaman and Spillmann had recommended it to me, as had also several of my old Danville classmates, including Duncan and Emmons. Bryan was there, and I wanted to study with him. Besides, Bloomington was only fifty miles from my home and living there was not too expensive. I had twelve hundred dollars in sight (as a loan) to keep me and my family for two years.

During my two years at Indiana University I took all of the psychology courses offered, a year of neurology, as much philosophy and education as I could get in, third and fourth year German, almost three years of French, a year of sociology and economics, and some miscellaneous courses. In the two years, including summer quarters, I managed by overwork to collect three and a half years of credit and to secure both the A.B. and the A.M. degrees. But I got something more important than grades and degrees. In the classes of Bryan, Lindley, and Bergström I became fired with the ambition to become a professor of psychology and to contribute something myself to the science. Bryan and Lindley were brilliant and inspiring teachers. Bergström was at first disappointing because of his modesty and lack of personal force, but his solid worth soon became evident. He was not only a wizard with apparatus, but an able experimentalist and a scholar. At the end of my first year Bryan was made President of the University and thereafter I had but one class with him.

My chief mentor from the very beginning was Lindley, and I could not have found a better. My indebtedness to him both for instruction and personal encouragement could hardly be overstated. Throughout my second year, while I was doing my master's thesis with him, he gave me an hour's conference every week. Often the hour stretched into two before we parted. Like Bergström, he had only recently returned from a post-doctorate year of study and research in Germany. Through them I was inspired to serious study of Wundt, Ebbinghaus, Kraepelin, and Külpe in Germany; Ribot, Tarde, Binet, and the Charcot school in France; Lloyd Morgan and Galton in England; and James Ladd, Hall, Sanford, Burnham, Cattell, Titchener, and Mark Baldwin in America. My courses with U. G. Weatherly, in anthropology, sociology, and economic theory, were invaluable adjuncts to my psychological program and helped to mold my life interests. In all my work I had the great advantage of being able to read both German and French fluently before the end of my first year. I studied with Bryan Falkenberg's *History of Philosophy,* Külpe's *Introduction to Philosophy,* and Royce's *Spirit of Modern Philosophy,* and read Ladd's *Philosophy of Knowledge* with a fellow student. In a course taught by L. C. Carson during my last year I read two volumes of Locke's philosophical works, Berkeley's *Theory of Vision* and *Principles of Human Knowledge,* Hume's *Enquiry Concerning Human Understanding,* Descartes' *Dis-*

course on Method, and most of Kant's *Critique of Pure Reason.* Of these I enjoyed all except Kant, who seemed to me so unnecessarily obscure. But my philosophical interests were rapidly waning in competition with my enthusiasm for psychology, and they never regained their former position.

In experimental psychology the texts were Sanford and Titchener (Volume I, *Qualitative Experiments*). Sanford's text was then but three years old and Titchener's was fresh from the press. Only twenty-two years had passed since Wundt had opened the first psychological laboratory. The newness of the subject had its appeal, but neither that nor the gifted teachers I had could make me enjoy working with apparatus.[4] The courses which especially stand out in my memory include the following: with Bryan, ethics, the history of philosophy, and a seminar course with individual reports; with Lindley, abnormal psychology, neurology, animal psychology, a seminar on James's *Principles,* and a seminar with individual reports, besides the personal conferences; with Bergström, experimental psychology, school hygiene, and the history (really the philosophy) of education.

The seminar reports which I prepared almost certainly influenced my later work more lastingly than any of the formal instruction I had. Two reports for Lindley, one on "Degeneracy" and the other on "The Great-Man Theory" caused me to read almost everything I could find in the library, in English, German, or French, on the psychology of mental deficiency, criminality, and genius. My choice of an experimental study of leadership for a master's thesis was influenced partly by the reading I did on these reports, partly by a study of Binet's recently published book on suggestibility, and partly by the articles of Hall and his students in the field of child study. The problem was one which gave a chance to do an experimental study without apparatus, to work with children, and to learn something at first hand about the rôle of suggestibility and imitation in leadership. As a scientific contribution, my master's thesis, although later published in the *Pedagogical Seminary,* was worthless; as a contribution to my personal development it fulfilled its purpose ideally.

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[4] Three years of work in the laboratory at Indiana and Clark Universities did not enable me to overcome my mechanical ineptness. The set-ups were always difficult for me and a piece of "machinery" always seemed to be an obstruction between me and the thing I was trying to get at. "Brass-instrument" psychology was all right for the other fellow, but was not intended for me. My dislike of apparatus doubtless had something to do later in turning me to tests and measurements of the kind that make no demands upon mechanical skill.

Among my classmates of the Indiana period who continued to the doctorate in psychology were B. W. DeBusk, M. E. Haggerty, James P. Porter, and Jesse Hayes White.

As my second busy year at Bloomington drew to a close and my funds became exhausted, I was filled with regret at the thought of having to leave my studies for a teaching position. An opportunity came to teach psychology and pedagogy at Central Normal College, but at a salary too low to enable me to pay my debts and save for further schooling. I began looking for a public-school position which would pay better, but before I found one, there came an offer of a fellowship from Clark University. I had allowed Lindley to recommend me for it, at his suggestion, with hardly a thought that it would really be offered me. In fact, I was greatly embarrassed when word came that I had been accepted; I had now two children, I was twelve hundred dollars in debt, and I did not know where I could borrow more even if I were reckless enough to risk it. But Lindley, Bergström, and Bryan all urged me to accept if I could possibly arrange it financially. I laid the situation before my father and brother and they loyally offered to back me to the extent of another twelve hundred dollars.[5] My wife courageously approved, and the die was cast. Then, when our boxes were packed for shipping, I received the offer of one of the best high-school principalships in Indiana. Had it come two weeks earlier I should certainly have accepted it, and a very different career would probably have been the result.

CLARK UNIVERSITY

When I went to Clark in 1903 it was still the American Mecca for aspiring young psychologists. As Bryan, Lindley, and Bergström were all Clark men, I had thought of no other university for my doctorate. Hall, Sanford, and Burnham were already old friends, for I had read nearly everything they had written and had heard them quoted almost daily in my classes. But I went with a humility that amounted almost to trepidation, expecting to find myself ill-prepared and at a disadvantage in comparison with the others I should find there. I was only partly reassured when Hall, in my first conference with him, let me know that, because of the "splendid training" I had had, and the "fine report" my Indiana teachers had given of me, he was expecting "great things" of me. Had I only known that this was a favorite pedagogical device of Hall's, I should have suffered less from a burdening sense of responsibility during the months that followed!

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[5] When I left Clark I was $2500 in debt, or about the equivalent of $5000 to $6000 at the present time.

The Clark of my day was a university different in important respects from any other that has ever existed in America, if not in the world—in spirit much akin to the German university yet differing from it because of the small student body. It enrolled in all its departments only about fifty full-time students, besides possibly a dozen whose attendance was limited to Saturday classes or special seminars. Possibly thirty of the fifty were there primarily for psychology, philosophy, and education. The informality and freedom from administrative red tape were unequalled. The student registered by merely giving his name and address to President Hall's secretary. He was not required to select formally a major or a minor subject. There was no appraisal of credentials for the purpose of deciding what courses he should take. *Lernfreiheit* was utterly unrestricted. There were professors who proposed to lecture and there were students who proposed to study; what more was necessary? The student could go to three or four lectures a day, or to none. No professor, so far as I could see, kept a class list. Attendance records were, of course, unheard of. No marks or grades of any kind were awarded at the end of the year or semester. One could attend a course of lectures all year without being required or necessarily expected to do the least reading in connection with it. There were no formalities about candidacy for a degree. The student was allowed to take his doctor's examination whenever the professor in charge of his thesis thought he was ready for it. No examination except the four-hour doctor's oral was ever given.

On first entering the University, the student was always advised by President Hall to sample all the courses he thought he might be interested in, and to drop those he cared least for. Students of psychology ordinarily began by taking most of the courses given by Hall, Sanford, Burnham, and Chamberlain, and perhaps a course by Hodge in neurology or physiology. Nearly all the courses started out with an attendance of twenty or thirty, which in some cases was reduced to ten or less before the middle of the semester. A course by Chamberlain in anthropology dwindled to four and finally to two.

It might have been one instead of two, only I had stayed too long to drop out without embarrassment.

A professor lectured only three or four times a week and on whatever subject he pleased. *Lehrfreiheit* was as unrestricted as *Lernfreiheit.* There was no effort to make the courses of different professors dovetail. Chamberlain gave only two lectures a week; Burnham three, besides a seminar; Sanford two or three in addition to his laboratory courses; and Hall usually four, besides his weekly four-hour seminar. Burnham's lectures were always read from manuscript and were finished products, ready for the publication which he always postponed. Hall talked from notes, the freshness or staleness of which the student could gauge by the amount of fumbling of papers and by the élan of his delivery. His lectures were like his writings in their wide sweep and their wealth of allusion. Sanford's, on the other hand, were limited in scope and were delivered in a matter-of-fact manner. Those of Chamberlain and Hodge were unorganized and sometimes rambling. One gathered that none of the professors except Burnham considered his lectures a particularly important part of his job. They were there primarily to carry on research and to guide students in research.

I think the Clark situation as I have described it was of almost crucial importance in my development. I have never worked well under the restraint of rules and regulations, and it is hard to imagine a régime that would have been better adapted to my temperament than the one I found at Clark, if régime indeed it could be called. Because I was placed absolutely on my own responsibility, I was able to give my best with unalloyed enthusiasm. It is a method which affects not only the quality of a student's work, but also the nature of his later output. It is conducive to intense concentration and monographic production, rather than to well-rounded scholarship and the production of systematized treatises of the textbook variety. Clark University can pride itself in the fact that hardly any textbooks have been written either by its professors or by its graduates.

The manner of conducting the library was in harmony with the general spirit of the University. The only formality was that books had to be signed for when they were taken from the building. There were no barriers between students and books. A book did not have to be signed for to be used in the library. Stacks and reading room were combined. Each student could have an entire alcove of perhaps ten by sixteen feet, and a large table, to himself. There he was able to work undisturbed with whatever books he took a fancy to; there he could do his writing if he wished. The library had been endowed separately from the University proper, and so magnificently that in those days it had difficulty in spending its income. No student ever wanted a book that the librarian would not gladly get, whatever the cost and in whatever language it was printed.

The laboratory facilities, in psychology at least, were hardly less generous in proportion to the demands upon them. There was unlimited room, and apparatus was available for almost any type of experiment a student might want to undertake. All this meant much less to me than did the library, where I spent so much more of my time. For me, Clark University meant chiefly three things: freedom to work as I pleased, unlimited library facilities, and Hall's Monday evening seminar. Any one of these outweighed all the lectures I attended.

When Clark students of the old days get together, their conversation invariably reverts to Hall's seminar. All agree that it was unique in character and about the most important single educational influence that ever entered their lives. No description could possibly do it justice; its atmosphere cannot be conveyed in words. It met every Monday evening at 7:15 and was attended by all the students in psychology, philosophy, and education; in my day about thirty in number. Each evening two students reported on work which had occupied the major part of their time for several months. Usually we knew in advance who would hold forth, and an air of expectancy was general. If the reporting student was one whose ability and scholarship commanded respect, we were prepared to listen and learn. If he was an unknown quantity or was regarded with suspicion, we were prepared to listen and criticize. The longer or more important report came first. It was always under way before 7:30 and might last an hour or longer. Ordinarily, though not always, it was read from manuscript. It might be either a summary and review of the literature in some field or an account of the student's own investigation. When the report was finished Dr. Hall usually started the discussion off with a few deceivingly generous comments on the importance of the material that had been presented, then hesitantly expressed just a shade of doubt about some of the conclusions drawn, and finally called for "reactions." Sometimes when we were most critically disposed Dr. Hall's initial praise of the report momentarily spiked our guns. Soon, however, a student bolder than the others would dare to disagree on some fundamental proposition; others would then follow suit, and the fat was in the fire. When the discussion had raged from thirty minutes to an hour, and was beginning to slacken, Hall would sum things up with an erudition and fertility of imagination that always amazed us and made us feel that his offhand insight into the problem went immeasurably beyond that of the student who had devoted months of slavish drudgery to it. Then we were herded into the dining room, where light refreshments were served, and by 9:30 or so we were in our chairs listening to another report. Sometimes the second half of the evening was even more exciting than the first half, and we rarely got away before eleven or twelve o'clock. I always went home dazed and intoxicated, took a hot bath to quiet my nerves, then lay awake for hours rehearsing the drama and formulating the clever things I should have said and did not. As for Dr. Hall, he, as I later learned, always went upstairs to his den and finished his day by reading or writing until 1:00 A.M. or later. So inexhaustible was his energy!

If there is any pedagogical device better adapted to put a man on his mettle than a seminar thus conducted, I do not know what it is. To know that his contribution would be subjected to merciless criticism from every angle was enough to arouse even a naturally indolent person to Herculean effort. In preparation for the report, the student was likely to cut all his lectures for a week or so and to reduce his sleep to half the usual amount. If the report met with general disapproval, it was sometimes followed by a collapse of nerves that would send the poor victim to bed; in one case, by a breakdown that necessitated several months of vacation.

Because of the small enrollment at Clark and the intimate associations which existed among the students, one's classmates were likely to be almost as potent a source of influence as the professors themselves. Among those who attended Clark either one or both of the years I was there were W. F. Book, A. A. Cleveland, Edward Conradi, Arnold L. Gesell, E. B. Huey, James Ralph Jewell, Fred Kuhlmann, Walter Libby, George E. Myers, Josiah Morse, James P. Porter, Jonathan Rigdon (my former teacher), John W. Slaughter, and Charles Waddell. Perhaps my most intimate associations were with Book, Conradi, Gesell, Huey, Kuhlmann, Morse, and Porter. Huey had taken his degree some years earlier and when he returned to Clark at the end of my first year he was fresh from Europe and much occupied with the possibilities of clinical psychology. He spent many evenings at my house and told me a great deal about developments in psychology on the Continent, and particularly about his contacts with Binet and Janet. Book was at work on his typewriting experiment, and, as he was from Indiana University, I naturally saw much of him. Conradi, who was also from Indiana, lived across the street from me and was doing an experiment on imitation in birds, in addition to his thesis on stuttering. Gesell, who was there only during my second year, was already developing an interest in the kindergarten period. I followed closely Kuhlmann's experiment in the psychology of mental deficiency and served for several months as a reagent in his experiments on memory. Porter had been laboratory assistant at Indiana University during my first year there and had preceded me at Clark by a year. He was completely engrossed in his experiments with birds. Cleveland had begun his experiment in the psychology of chess and for a time carried on daily observations of my infant daughter. Morse had taken his degree the year before I arrived but remained for the two years I was there. Slaughter had taken his doctor's degree with Wenley at Michigan in 1901, and was docent at Clark 1901-1903 and 1904-1905. He was regarded by all of us as by far the ablest member of our group, but he later abandoned psychology.

All of these men helped to give direction to my thinking and their combined influence was doubtless very potent. If my debt to Huey and Kuhlmann has been more lasting than in the case of the others, this is due in part to the increasing similarity of our interests in the years that followed. All in all, the group at Clark between 1903 and 1905 averaged high. The names of at least twelve are found in the 1928-1929 edition of *Who's Who.* As a good many of the group went into education, I find only seven names in *American Men of Science* (1927), of whom two are starred as in the first fifty of American psychologists. Huey, one of the most promising for science, died in 1913.

Besides the students I have mentioned, there were others of excellent ability whose interests were more remote from my own. There were still others who composed what some of the students referred to as the "lunatic fringe," for which Clark in the old days was noted. There was a semi-psychotic Swede who had ridden the trucks of freight trains for three thousand miles in order to study with Hall, only to find himself the imagined victim of dreadful persecutions by Hall and others. There was a tradesman of more persistence than brains who had somehow glimpsed the higher intellectual life and had been struggling for years to win his doctorate. There was a foreign "university tramp" who had already taken three Ph.D.'s in as many different subjects and was then in pursuit of his fourth—the perfect example of a man educated beyond his intelligence. There were oldish spinsters who made up in enthusiasm for child study what they lacked in feminine charm. Hall's lectures and writings seemed to have exuded a ferment that took effect in all kinds of soil.

Like many other students who went to Clark in those days, I was drawn there largely by the inspiring effect of Hall's writings. I remained pretty much under his hypnotic sway during the first half year. At his suggestion, I made a survey of the literature on precocity and, after some protest, inflicted upon the world a questionnaire on leadership among children; but before the end of the year I had had enough of the questionnaire method. In my effort to find a solid footing for research with gifted and defective children, I was becoming more and more interested in the method of tests and was reading almost everything that had been written on the subject, including the works of Galton, Binet and his collaborators, Bourdon, Oehrn, Ebbinghaus, Kraepelin, Aschaffenburg, Stern, Cattell, Wissler, Thorndike, Gilbert, Jastrow, T. L. Bolton, Helen Bradford Thompson, Spearman, Sharp, and numerous minor contributions to the growing literature of the field. By the spring of 1904 I had determined to take as my thesis an experimental study of mental tests. Hall had been so kind to me and I owed him such a debt of gratitude that it cost me a heavy soul-struggle to desert him in favor of Sanford as my mentor. When I announced to him my decision he expressed very emphatically his disapproval of mental tests, but, finding that my mind was made up, he finally gave me his blessing and some advice on the danger of being misled by the quasi-exactness of quantitative methods.

It may be of interest to review briefly the situation in America with respect to mental testing when I was planning my thesis investigation in the summer of 1904. Gilbert had published, in 1894, his "Researches on the Mental and Physical Development of School Children," Cattell and Farrand, in 1896, their report on "Physical and Mental Measurements of the Students of Columbia University," Miss Sharp, in 1899, her research on "Individual Psychology," Wissler in 1901, his research on "The Correlation of Mental and Physical Tests," Miss Thompson, in 1903, her "Psychological Norms in

Men and Women," and Thorndike, the same year, his monograph on "Heredity, Correlation, and Sex Differences in School Abilities." These were the major experimental studies of mental tests that had been published in America. Kuhlmann's study of the feebleminded was in press. Thorndike's study of twins did not appear until 1905, but the first editions of his *Educational Psychology* and his *Introduction to the Theory of Mental and Social Measurements* were published in 1904.

Spearman's two notable contributions of 1904 came too late to have much influence on my thesis plans; and, even if they had come earlier, it is doubtful whether my equipment and point of view would have enabled me at the time to profit greatly from them. I shall never forget, however, the impression that those articles made on me—the dogmatic tone of the author, the finality with which he disposed of everyone else, and his one-hundred-per-cent faith in the verdict of his mathematical formulae. I read both articles through several times, or all that I could understand of them, and was left in a state of suspended judgment. The author's logic appeared to be waterproof, but the conclusion to which it led, namely, that there is "a correspondence between what may provisionally be called 'General Discrimination' and 'General Intelligence' which works out with great approximation to *one* or *absoluteness,*" seemed to me as absurd then as it does now. But, if Spearman's logic failed to convince me, the originality of his attack commanded my utmost respect. The impression which Thorndike made on me up to 1905 was somewhat similar, though less extreme. I could understand him better than I could understand Spearman, but my admiration of his independence was tempered a little by the cocksureness with which he tore into "established" psychological doctrine. For a youth still in his twenties, he seemed to me shockingly lacking in a "decent respect for the opinions of mankind!"

My own interest in mental tests at the time was more in their qualitative than in their quantitative aspects. I wanted to find out what types of mental processes are involved in the thing we are accustomed to call intelligence. I therefore selected two groups of subjects of nearly the same age, a "bright" group and a "dull" group, and proceeded to look for tests that would bring out differences in their performances. I did not then realize the extent to which this is dependent upon the intellectual level of the subjects, and I did not fully appreciate the significance of age norms of performance.

After I began the experimental work on my thesis near the end of the year 1904, the testing of my subjects occupied the greater part of my time (some five hours a day) until the following May. The thesis represents, on the whole, the best I was able to do with the equipment I possessed. Whatever its merits or its faults, it was my own work. I selected the problem, devised the tests which I proposed to use, decided almost every detail of procedure, and wrote up my results unaided. The problem was outside Sanford's field; he followed the progress of the experiment with friendly interest but rarely ventured a suggestion.

Everything considered, the Clark period was even richer in experience and stimulation than I had expected. Hall and Burnham were then at their best. If I had suffered some disillusionment about Hall as a scientist, this was more than made up by the burning enthusiasms which he inspired. Burnham's scholarly and well-rounded lectures gave me a splendid systematic orientation in the field of educational psychology and provided a solid foundation on which to build. His courses in school hygiene and the hygiene of instruction deeply influenced my reading and teaching for more than a decade. From the somewhat erratic but erudite Chamberlain I got little directly, though, partly as a result of my contacts with him, I browsed rather extensively in anthropological literature. The courses I took with Hodge were of limited value, but I got more from his seminar and still more from my personal associations with him. Sanford, the ablest scientist of the group, was something of a disappointment. That he should convert me to the laboratory was hardly to be expected, but I got far less both from his laboratory courses and from my other associations with him than I felt I had a right to expect. In this I was not alone. The truth was that Sanford's tide of interest in experimental psychology was then ebbing. Whether he had lost faith in its value or whether he was in the throes of an inner crisis of some other origin, I do not know. It was probably the latter, for he seemed to have a particularly inhibited personality and was subject to nervous fatigue and states of depression. Nevertheless, Sanford was a man of rare charm and fineness of soul. His scientific objectivity and keen, critical judgment on psychological issues and methods gave us great respect for his ability.

One thing that I much needed and that Clark did not have to offer was instruction in statistical methods. It would have been an untold boon to me if I could have had a year with Thorndike immediately upon leaving Clark; but there were no post-doctorate fellowships in those days.

In thinking over the work of leading psychologists, I have been struck by the similarity between their earlier and their later work. There are exceptions, of course, but in the majority of cases the similarity stands out clearly with respect to field of interest, method of work, psychological views, and literary expression. Each man's career shows certain lines of development, but in directions that could almost have been predicted. The Hall of 1880 was essentially the Hall of 1920; the Kraepelin of 1885, the Kraepelin of 1915; the Binet and Titchener of 1890, the Binet and Titchener when they left us; the Spearman and Thorndike of 1900, the Spearman and Thorndike of today. This is not less true of the psychologists I knew in the making at Clark University. As for myself, everything I have done since 1905 was foreshadowed in my interests at that time—in the psychology of genius, the measurement of intelligence, the phenomena of individual differences, in general, and the problems of hygiene. The small progress one makes after the age of twenty-five or thirty toward higher levels or new fields of achievement is a hard blow to one's pride; it would be so much pleasanter to think of oneself as capable of unlimited growth in any direction. One is reminded of a remark that Samuel Johnson made when he had reached the age of fifty-seven: "It is a sad reflection, but a true one, that I knew almost as much at eighteen as I do now."

The Fallow Period

In the summer of 1904, following my first year at Clark, I suffered a pulmonary hemorrhage, the third warning I had had of trouble in that quarter. Previously, in 1899 and in 1900, the diagnosis had been uncertain, but this time there was no doubt of a mildly active tuberculosis. It was probably fortunate for me that the doctors of that time knew so little. According to present practice, I should have been put to bed for six months or a year, in which case I should probably have gone to pieces from worry over my broken plans and the mounting burden of debt. As it was, I merely rested for a couple of weeks until my temperature had subsided and then went back to work. During my last year at Clark, however, I led a more careful life, avoiding fatigue and giving special attention to diet and sleep. By the end of the year my condition had greatly improved, but my physician warned me that it would be desirable to seek a more congenial climate, and I therefore limited my search for a position to the South and Southwest. Late in the spring, after some months of anxious waiting, the friendly fates brought me three offers in as many days. The first was the presidency of a struggling normal school in St. Petersburg, Florida, which I accepted. Then, within forty-eight hours, I was offered a one-year position at the University of Texas as substitute for Professor Caswell Ellis on half salary, and also the principalship of the high school at San Bernardino, California. After two sleepless nights of indecision, health considerations won out over professional ambition and I secured release from the Florida position in order to go to California. It would be useless to speculate what my future would have been if I had gone to Florida or Texas. The Florida position was offered to Conradi, who accepted it and later became President of the Florida State College for Women. The Texas position went to Morse, now Professor of Philosophy at the University of South Carolina.

My year at San Bernardino was a busy and happy one. Shortly after school opened there was another threat to my health, but by the end of the year I was again in fair condition and was looking forward contentedly to a second year in the same position. Then, quite unexpectedly, there came a telegram from C. J. Albert, of the Albert Teachers Agency, saying that his old friend Dr. Millspaugh, President of the Los Angeles State Normal School, wanted a man in my line. I took the next train to Los Angeles, had a conference with Dr. Millspaugh, and in a few days was offered the position as Professor of Child Study and Pedagogy.

I was at Los Angeles for four years. The library facilities were unusually good for a normal school, the work was not too heavy, and the associations were pleasant. Among my colleagues in psychology and education during a part or all of my four years were Dr. Jessie Allen (now Mrs. W. W. Charters), Dr. Arnold Gesell, Beatrice Chandler (now Mrs. Gesell), Everett Shepardson (since deceased), and Dr. Wayne P. Smith—a group that would have been hard to match in any normal school in the country. All were scientifically minded and intellectually stimulating. Without them, the years I spent in Los Angeles would have been far more arid than they were. The insecurity of my health, which made it seem unwise to undertake any more work than the minimum my teaching demanded, rendered these associations especially important. I read only moderately, tried to forget that I was ever interested in research, and spent as much of my time as possible out of doors.

The contacts which meant most to me both professionally and personally during this period were those with Gesell and Huey. For two years the Gesells lived across the road from me near the foothills, and we were much together. The vacation of 1907 I spent with my family in the San Bernardino mountains, where Huey was our guest for the greater part of the summer. He was becoming more and more interested in clinical psychology and our daily talks concerned chiefly mental examination methods. In 1910 Huey again visited me, this time for two weeks. It was just before the beginning of my first year at Stanford; I was "boning" on the courses I was to give and was naturally in a receptive state of mind. At this time he told me much about the work he had been doing in Adolf Meyer's clinic at Hopkins and about the work of Binet and Goddard. He urged me to start some work at once with the Binet 1908 scale for measuring intelligence.

STANFORD UNIVERSITY

As the reader has seen, there were many links in the chain of events which brought me to Stanford. Chance played its part in my leaving a public-school position to attend Indiana University, in my going from there to Clark instead of to a high-school principalship, in my move to California, and again in my hearing of the Los Angeles position in time to apply for it. In 1909, Bergström, my former teacher, was called from Indiana to a newly created professorship of educational psychology in the Department of Education at Stanford, but before the end of the first year his life was prematurely ended. The position was then offered to Huey, who refused it in order to continue his clinical researches with Meyer and recommended my appointment instead. Thus, in 1910, I found myself a member of the faculty of Stanford University, the university that I would have chosen before any other in all the world.

Nothing could have been more fortunate for me than the call to Stanford at this particular time. I had regained my health and was becoming restless. Gesell had left for Yale a year earlier, but I was compelled to wait until a call came from the right climatic location; for I was unwilling to risk a position in the East or Middle West. Five years had passed since I left Clark, and I had reached the age of thirty-three. A few more years of waiting and my chances of a good university position would have begun to dwindle.

Professor Elwood P. Cubberley, who brought me to Stanford and was my "chief" from 1910 to 1922, gave me every opportunity and encouragement. Although my initial rank was only that of assistant professor, my teaching schedule was light. Insofar as the needs of the Department permitted, I was given free range in the selection of courses anywhere in the field of educational psychology and mental development. In collaboration with one of my graduate students, H. G. Childs, I began at once an experimental study of the Binet tests and continued with this problem until the publication of *The Measurement of Intelligence* in 1916. In the meantime, through my courses in school hygiene and by writing *The Hygiene of the School Child,* I was giving vent to another of my deep-seated interests. This had its origin with Lindley and Bergström at Indiana, gained new life from Burnham's lectures at Clark, and was given imperative need for expression by my personal health problem. There is an old saying that if you scratch a health reformer you will find an invalid.

The Intelligence of School Children and the Warwick and York monograph on the Stanford Revision data were by-products of the work that led to *The Measurement of Intelligence.* I was a little surprised that my publications in the test field were so favorably received. I knew that my revision of Binet's tests was superior to others then available, but I did not foresee the vogue it was to have and imagined that it would probably be displaced by something much better within a few years.

Apart from the possible fate of my own work, I did not expect mental tests to gain acceptance nearly so quickly as they did. I was quite aware of the fact that many of the old-line psychologists regarded the whole test movement with scorn. I was probably more sensitive on this point than the facts warranted, with the result that I made at this time fewer contacts with psychologists in other fields than I should have done. Between 1910 and 1916 I made no trips East and did not even apply for membership in the American Psychological Association. I had the feeling that I hardly counted as a psychologist unless possibly among a few kindred souls like Gesell, Goddard, Kuhlmann, Thorndike, Whipple, Yerkes, and a few others who had become "tarred by the same brush." With these I kept in as close touch as possible through publications and sporadic correspondence. In 1916, I taught in the summer school at New York University and the following year in that at Columbia. Both experiences were exceedingly stimulating.

Among psychologists whose works I studied most assiduously at this period were Janet, Meumann, Spearman and his followers, Stern, Ebbinghaus, Thorndike, Whipple, and writers of the Freudian school. I read again all of Binet's earlier contributions on mental tests, lectured on the work of Stern and Meumann, published a fifty-page summary and criticism of Meumann's *Experimentelle Pädagogik,* and took note of most of the theses that appeared from Chicago, Clark, and Columbia. I read regularly almost everything in *l'Année, The British Journal, Zeitschrift für angewandte,* Kraepelin's *Psychologische Arbeiten, Zeitschirft für pädagogische Psychologie,* and all the American journals. It was Thorndike whose writings stimulated me most at this time, perhaps because I found myself in almost perpetual disagreement with him. Next in order would probably be Meumann, Stern, and Whipple.

At this time I also made careful notes of all the important monographs on mental development and reread Ellis, Krafft-Ebing, and others on the psychology of sex. I read Titchener's books and articles as they appeared, but was little influenced by them. What we are accustomed to think of as "Titchenerian psychology" has always appeared to me as singularly sterile.

Then came the War, with service on the committee that devised the army mental tests; on Yerkes' staff, first, as Director of Research on the army tests and later as collaborator with Yerkes, Boring, and others on the historical account of psychological work in the army; and as a member of Scott's Committee on Classification of Personnel. It would take us too far afield to enter into the new world of experiences which the war work opened up to me. Their most important aspect, so far as my personal development is concerned, was in the opportunity they gave me to become acquainted with nearly all of the leading psychologists of America. Among the war associations which meant most to me were those with Yerkes, Thorndike, Whipple, Scott, Woodworth, Kelley, Bingham, Yoakum, Mabel Fernald, Bridges, Boring, Dodge, Goddard, Strong, Wells, and May. Through them and others my information was extended and my interests broadened in many fields of psychology. My intimate contacts with Yerkes in particular, both in our daily work and during the long periods when I lived in his home, meant more to me than could easily be expressed.

One result of the war experiences was to confirm and strengthen my earlier beliefs regarding the importance of mental tests as an integral part of scientific psychology. Whereas I had thought that only a handful of psychologists were of this opinion, I now learned that many were. I no longer felt isolated. I could return to my work with more confidence than ever that, in the long run, contributions in the field of mental tests would receive the recognition they deserved.

For a couple of years after the War a good part of my time was devoted to work on various kinds of tests, including *The National Intelligence Tests, The Terman Group Test of Mental Ability,* and *The Stanford Achievement Tests.* Then, through a liberal grant from the Commonwealth Fund, came the long-wished-for opportunity to undertake a major research with gifted children, a field in which I had been working for some time. The result was *Genetic Studies of Genius,* Volume I appearing in 1924, Volume II (by Dr. Catherine Cox) in 1925, and Volume III (by Burks, Jensen, and Terman) in 1930. *Children's Reading* (by Terman and Lima) was largely a by-product of the gifted children study.

In 1922 I was transferred to the Department of Psychology as successor to Professor Frank Angell, who had headed the Department since its beginning in 1892 and was retiring as Professor Emeritus. Thomas Welton Stanford had recently left approximately a half million dollars to the University for psychology, and this fund, which had just become available, offered a rare opportunity to develop in a short time a strong psychology department. The first two positions filled were those in experimental psychology and animal behavior, the former by Miles and the latter by Stone. A year later Strong was added in vocational psychology. Coover and Merrill were already members of the Department, Coover in learning and psychical research and Merrill in clinical psychology. On my recommendation, Kelley had been brought to Stanford in 1920 as Professor of Education, before my transfer to the Department of Psychology, and arrangements were later made whereby he served as a member of both the education and the psychology faculties. Farnsworth was added to the faculty in 1925. My judgment in the case of all these men was well borne out by their later achievements, and Stanford quickly assumed a position of leadership in psychology. Although I dislike doing work of an administrative nature, I think that nothing I have accomplished has given me a stronger or juster sense of pride than my part in helping to build up an outstanding department at Stanford. The task was made easier by the encouragement and unfailing support of President Wilbur and by the high standards of teaching and research which Professor Angell had maintained since the foundation of the Department.

During six years I served as a member of the University Scholarship Committee and, in this capacity, I was instrumental in bringing about the introduction of intelligence tests as a partial basis for the selection of candidates for admission to the University and brought about the establishment of the Independent Study Plan for gifted students.

Both before and since my transfer to the Department of Psychology I have been fortunate in the graduate students who have elected to work with me. Among those for whose doctor's theses I have been mainly or solely responsible are (in approximate order of date): J. Harold Williams, Arthur S. Otis, Samuel C. Kohs, William T. Root, James R. Stockton, Giles M. Ruch, Maud A. Merrill, Kimball Young, Virgil E. Dickson, Curtis E. Merriman, Marvin L. Darsie, Vernon S. Cady, Florence L. Goodenough, Albert S. Raubenheimer, James C. DeVoss, Ellen B. Sullivan, Catharine Morris Cox, Raymond R. Willoughby, Dortha Williams Jensen, Helen Davidson, Barbara Stoddard Burks, and Robert G. Bernreuter.[6] It is perhaps indicative of my own concentration of interests that all but three of these theses belong in the field of individual differences. The three exceptions are in learning (Ruch, Sullivan, Davidson), and two of the three deal with the relation of learning to intelligence (Ruch, Davidson). Of the others, three have to do with intelligence test construction (Otis, Kohs, Goodenough), one with the nature of intelligence (Stockton), one with the intelligence of delinquent subjects (Williams), three with the construction and validation of character or personality tests (Cady, Raubenheimer, Bernreuter), two with race differences (Young, Darsie), four with gifted children (Root, DeVoss, Cox, Jensen), three with family resemblances in mental abilities (Merriman, Willoughby, Burks), one with school achievement of defectives (Merrill), and one with the classification of school children for instruction (Dickson). Of two post-doctorate Fellows whose researches I have sponsored, one, Dr. Keith Sward, tested the intellectual abilities of young and old professors; the other, Dr. Harold Carter, made a particularly detailed study of the resemblance between identical twins in mental abilities and personality traits.

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[6] In the case of several of these the responsibility was shared by Kelley (Merriman, Cady, Raubenheimer, DeVoss, Willoughby, Jensen), in two cases by Miles (Sullivan and Davidson), and in two cases (Young and Dickson) by professors in the Department of Education.

To these students and to many others who did not go so far as the doctorate, or for whose theses I was not primarily responsible, I am enormously indebted. All, in one way or another, have influenced my interests, and some of them have led me to extensive modification of my views. I think I can truthfully say that I have always tried to encourage the graduate student to do his own thinking, and that, other things equal, I remember with most satisfaction and gratitude those who held out most strongly for their opinions.

My own research activities of the last five years have been confined chiefly to three projects: a follow-up study of a thousand gifted children (Volume III, *Genetic Studies of Genius*), a study of sex differences in non-intellectual mental traits, and a new revision of the Stanford-Binet intelligence tests. On the first I have had as chief collaborator Barbara Stoddard Burks; on the second, Catharine Cox Miles; and on the third, Maud A. Merrill. The last two studies have been under way for three years and are still unfinished. My interests at present are largely in the fields of personality testing, mental inheritance, and the psychology of genius.

The Editor has suggested that it would be interesting if the author of each of these autobiographies would render some sort of appraisal of his own work. He did not say whether he thought it would be enlightening as well as interesting! I recall that Binet pointed out, long ago, the difficulties of auto-criticism and the risk it imposes of giving oneself away; nevertheless, the invitation is accepted.

I am fully aware that my researches have not contributed very greatly to the theory of mental measurement. On problems of less theoretical significance, but of importance for the usefulness of tests and for the psychology and pedagogy of individual differences, I think I have made contributions of value. If I am remembered very long after my death, it will probably be in connection with my studies of gifted children, the construction of mental tests, and the psychology of sex differences. I think that I early saw more clearly than others the possibilities of mentality testing, have succeeded in devising tests that work better than their competitors, and, by the application of test methods, have added to the world's knowledge of exceptional children. My contributions were greatly favored by my early interests, the opportunities which my Stanford position provided, and the persistence with which I have applied my efforts in a particular field.

In response to the Editor's request for a statement of my position with reference to current psychological issues and movements, I venture to offer the following credos, which range all the way from tentative beliefs to fairly positive convictions:

That mental testing is in its merest infancy and will develop to a lusty maturity within the next half century; that its developments will include improved tests of general intelligence (in the reality of which I believe), tests of many kinds of special ability, and tests of personality traits which no one has yet even thought of measuring;

That within a few score years school children from the kindergarten to the university will be subjected to several times as many hours of testing as would now be thought reasonable;

That educational and vocational guidance will be based chiefly on test ratings, and that Hull's proposal to measure every important ability and personality trait and to "grind out" a hundred or more occupational success predictions for every youth is practicable and will be realized;

That public vocational testing bureaus, employing methods of the kind referred to in the preceding paragraph, will be operated for the benefit of adults of all ages and both sexes;

That it will some day be possible to identify, largely by means of tests, the pre-delinquent and the pre-psychotic, and that effective preventive measures will result from this advance;

That matrimonial clinics will become common and that couples in large numbers will submit themselves to extensive batteries of ability, personality, interest, and compatibility tests before deciding to embark together;

That mental testing is destined to exert a profound influence on economic theory, industrial methods, politics, and the administration of law;

That the major differences between children of high and low IQ, and the major differences in the intelligence test scores of certain races, as Negroes and whites, will never be fully accounted for on the environmental hypothesis;

That mental testing will be more and more recognized as an integral part of experimental psychology, and that this recognition will be reflected increasingly in undergraduate instruction;

That psychiatry will not be pulled out of the mire until it lays down the requirement of two or three solid years of training in psychology, including psychobiology, mentality and personality testing, and statistical methods;

That psychology will, in time, give us a new type of biographical literature in which the interpretation of a subject's life will be based largely upon quantitative measurements of abilities, personality traits, and interest-attitudes;

That contrary to what would be suggested by an examination of the courses in teachers colleges and schools of education, psychology offers almost the sole basis for a science of education;

That the revival of associationism and the vogue given it during the last quarter century by the "bond" psychologists has about run its course;

That the Watsonian brand of behaviorism is a cult, and that its presumption in claiming the whole of psychology and in basing a theory of child training and a denial of heredity on a few minor experiments in the emotional conditioning of infants is ridiculous;

That the method of introspection has not been and never will be rendered obsolete by objective psychology, and that much greater use should be made of it in experimental learning and mental test construction than is customary at present;

That Gestalt psychology, even though its formulations are largely a matter of renaming old concepts, is exerting a wholesome influence on experimental work and on psychological theorizing;

That animal psychology is extremely important because of the greater opportunity it affords, in comparison with human psychology, of securing crucial data on certain types of problems in the fields of learning, mental inheritance, and the relation of intelligence and instinct to neural functioning;

That the Freudian concepts, even when their validity has been discounted about 90 per cent, nevertheless, constitute one of the two most important contributions to modern psychology, mental tests being the other.

I will close with a paragraph of "prejudices." On the Goodwin Watson test of Fair-Mindedness I rate as a rather extreme radical in my attitude toward the church and toward most problems of social ethics, but as merely a liberal on political and economic issues. On the whole, I am inclined to be pessimistic about present trends in democracy. I have a violent antipathy to prohibition, censorship, and most other activities of the moral reformers. I look upon Havelock Ellis as one of the most civilizing influences of the last hundred years. My taste in non-professional reading runs to fiction and biography. In fiction I prefer realism, and I like my biographies to give the kind of information that can be used as raw material for character analysis. The Strong Test of Occupational Interest rates me as having interests typical of psychologists and educators (Score A), moderately like those of personnel managers and journalists (Score B), and quite unlike those of artists, chemists, engineers, architects, farmers, and salesmen (Score C). On my test of mental masculinity and femininity I rate at about the average for men. On the Bernreuter Personality Inventory I rate at the ninetieth percentile in introversion and at the thirtieth percentile in dominance (aggressiveness). I dislike psychologists who exhibit over-much zeal in defending their pet systems. Of the founders of modern psychology, my greatest admiration is for Galton. My favorite of all psychologists is Binet; not because of his intelligence test, which was only a by-product of his life work, but because of his originality, insight, and open-mindedness, and because of the rare charm of personality that shines through all his writings.

MARGARET FLOY WASHBURN

SOME RECOLLECTIONS

Nothing gives the writer of the following paper courage to present it but the fact that she herself can read with interest the autobiography of anything human. Even this thought is hardly relevant, for an account merely of one's intellectual life can hardly avoid depicting a prig rather than a human being. Nevertheless, the temptation not to be left out of the autobiographical enterprise is irresistible.

There are progressive persons, interested in educational theory, who love to describe the defects of their own early training, but I seem to remember chiefly what was helpful in mine, so that, like Marcus Aurelius, I begin my meditations by thanking the gods for having given me "nearly everything good."

I was born in New York City on July 25, 1871, in a house built for my mother's father. It stood surrounded by a large garden, on a tract of land belonging to my mother's maternal grandfather, Michael Floy, which originally extended from 125th to 127th streets and from Fourth to Fifth Avenues. At the time of my birth both sides of 125th Street from Fifth Avenue to the Hudson River were occupied by white-painted frame mansions set in gardens. This great-grandfather of mine came from Devonshire and had won success as a florist and nurseryman in old New York. I have reason to thank the gods for his diligence, which enabled me to finish my professional training without having to earn my own living. All my other ancestors were in America before 1720; one-fourth of them were Long Island and Westchester County Quakers, five-sixteenths New York Dutch, one-fourth Marylanders, and one-sixteenth Connecticut Yankees.

I was an only child, and the first eight years of my life were spent in the Harlem house; my father then entered the Episcopal ministry and for two years had a parish at Walden, an Orange County village. We next moved to the small Hudson River city of Kingston, where I got my high-school training and whence I went to Vassar.

It seems to me that my intellectual life began with my fifth birthday. I remember a few moments when I was walking in the garden; I felt that I had now reached an age of some importance, and the thought was agreeable. Thinking about myself was so new an experience that I have never forgotten the moment.

I was not sent to school until I was seven, but, like many other persons, I cannot remember the time when I could not read, nor when I learned. The first school was a private one kept by the Misses Smuller, the three accomplished daughters of a retired Presbyterian minister who lived in the next house. It would be hard to find better teaching anywhere at the present time. In my year and a half there I gained, besides the rudiments of arithmetic, a foundation in French and German that saved me several years in later life, and the ability to read music and play all the major and minor scales from memory, a musical grounding that has been the chief aid to one of my greatest sources of enjoyment.

When we left New York for the two-years' sojourn in Walden, my school was, though still a private one, much like the district-school type, housed in a single-room building. I learned very little there: some American history and a little elementary physics. During these two years, between the ages of eight and ten, I wrote stories, of which one or two examples remain. They display no literary talent whatever except a precocious vocabulary, due to my constant reading. A family legend, by the way, was that the subject of this autobiography, aged seven, having had a bad tumble at school and been established as an invalid for the rest of the day, described the behavior of a playmate in the following impressive terms: "And Enid stood rooted to the spot with amazement at beholding me comfortably established on the sofa." Besides children's books such as the immortal *Alice*—in which the only thing I found funny was Alice's play with the black kitten before she went through the looking glass: the rest was highly interesting but not at all amusing—, George MacDonald's enchanting *The Princess and the Goblin,* which kept me awake the night of my seventh birthday and was read to pieces; all of Miss Alcott, Susan Coolidge, and Sophie May, I read between the ages of nine and twelve the whole of Dickens and the Waverley Novels.

The removal to Kingston came when I was eleven; here I entered a public school. By a blunder I was put into a grade too high for me, and suffered much anguish with arithmetic; in the spirit of M. Aurelius, however, it may be said that this was a piece of good fortune, for, managing somehow to scramble through the Regents' examinations, I entered the high school at twelve. New York State's system of Regents' examinations is, I believe, considered by all enlightened educators as below contempt, but I had much reason for gratitude to it. The terrifying formalities attending these examinations, where one's teachers with trembling fingers broke the seals on the packages of question papers sent from Albany, and one signed at the end of one's production a solemn declaration of having neither given nor received help, made all subsequent examinations in college and university seem trivial. What could be more comfortable and less awe-inspiring than being examined by one's own instructors?

The curriculum at Ulster Academy covered three years and would deeply distress a modern authority. It consisted of short-term courses in a large variety of subjects, each of which supplied a certain number of "Regents' credits." This method gave very poor results in the sciences, and my entire class failed twice to pass the Regents' examination in chemistry, having had no laboratory work. Our teacher performed some demonstration experiments, of which I can remember only sodium scurrying over the surface of water as a little silver ball and potassium bursting into flame under similar circumstances; also Prince Rupert's drop falling into dust when its tip was pinched; why, we had not the slightest idea. However, the course in "political economy" firmly fixed in one's mind the rudiments of the theory of supply and demand, and that in "civil government" equipped one with some lasting idea of the structure of state, county, township, and city. We had to learn the Constitution of the United States thoroughly, and a few years ago I was able to impress my colleague of the Department of Political Science at Vassar by answering test questions on it. Passing Regents' examinations in Latin had somewhat the nature of a sporting event. Having read four books of Virgil, we tried the examination on all six, reading at sight the passages from the last two. Several of us got over this hurdle, and the *Aeneid* knew us no more. What we lost in literary appreciation was gained in confidence for sight reading.

During these years I read in all my spare moments. Never having to do any school work at home, and enjoying the blessed privilege of an only child to be undisturbed when at leisure, I devoured all of Thackeray and Fenimore Cooper, Irving, Don Quixote (illustrated by Doré), Cary's translation of the *Divine Comedy* (similarly adorned), and a wide range of other literature including *Gulliver's Travels,* Fox's *Book of Martyrs,* and what I could make out of the

Canterbury Tales. There was a good library at home and another at the Academy. Scott was read and reread until I was fourteen, Dickens I read until about fifteen years ago, when his world began to seem too remote. That so much reading did no harm to health is shown by the fact that until I was twenty-six I was never ill. We had little in the way of out-door sport except skating, which came naturally to all dwellers by the Hudson.

In the spring of 1883 my parents and I made a memorable trip down the Mississippi from St. Louis to New Orleans by one of the old "palatial" steamers, which took a week for the run. I can still hear the call of the man with the lead, repeated from an upper deck and from the pilot-house, "Mark three!"; when it was "Mark twain!" a deep bell sounded once, the slow alternating puffs of the two engines stopped, and the great boat floated softly on over the shoal. The summer of my fourteenth birthday we went abroad for six weeks spent in the British Isles and in Paris; Walter Scott had made an excellent background for this journey.

I entered Vassar in the fall of 1886 as a preparatory student, for I lacked some Latin and had had no French since my earliest school days. Miss Smuller had laid so good a foundation that I needed only a semester at Vassar to secure admission to freshman French.

At this time there were no 'majors' in the Vassar curriculum. English, mathematics, Latin, a modern language, physics and chemistry, were required through the sophomore year; psychology, so-called, and ethics in the senior year; there was no requirement of continuity in any other subject. So far as there was continuity in my own studies, it lay in chemistry and French. Professor LeRoy Cooley taught chemistry and physics in crystal-clear lectures: his favorite word was 'accurate,' which he pronounced 'ackerate,' and I have loved, though by no means always attained, 'ackeracy' ever since. Particularly delightful was quantitative analysis, with the excitement of adding up the percentages of the different ingredients in the hope that their sum might approach one hundred; though the faint suspicion always remained that a particularly 'ackerate' result was due to losing a trace of something here and getting in a grain or two of dust there. French was admirably taught by two alternately kindly and ferocious sisters, Mlle. Achert and Mme. Guantieri, known to the students as Scylla and Charybdis: from the beginning no English was ever heard in the classroom, an unusual requirement in those days.

I am rather glad that I took no courses in English literature. When I was sixteen I began to love poetry, especially Keats, who absolutely bewitched me. Later, through a growing interest in philosophy, Matthew Arnold, with his matchless combination of classic beauty, clear thinking, and deep feeling became my favorite; I wrote my Commencement oration on "The Ethics of Matthew Arnold's Poetry," tracing the Stoic elements in it. For the love of poetry and philosophy I found in my sophomore year a strong stimulus in an older student who had been a senior in my preparatory year and had returned to college to work for a master's degree. She had been the leader of a brilliant group of girls in the class of '87, whose religious radicalism had distressed President Taylor in his first year of office. I now experienced the mental expansion that comes with dropping orthodox religious ideas, an expansion accompanied by exhilaration.

From my junior course in English I remember gratefully a book called *Rhetorical Analysis,* by Professor Genung of Amherst. It consisted of selections of prose from a wide range of masters; at the bottom of each page were detailed questions on the style, which we answered in writing; such as, "Exactly what does each of these metaphors contribute?" "Why is 'which' used instead of 'that' here, although 'that' is more nearly correct?" This work was invaluable in developing the ability to say what one meant and I recall it every time I try to write.

A wonderful new field was opened in my junior year by a course in biology whose teacher was a young Bryn Mawr Ph.D., Marcella O'Grady. She later married Theodor Boveri, the great authority on cytology, and has now, some years after his death, returned to America and to teaching. She lectured admirably and drew beautiful figures on the board. In this year, too, I began the study of Greek: Professor Abby Leach was a skillful teacher of its grammar, and brought the little group of my classmates in two semesters to the point where they could join the incoming freshmen who had had two years' preparation. I cherish proudly the scraps that remain, and pity the person who has to master scientific terms with no knowledge of Greek.

It was, I think, the summer after my junior year that I read in my father's library Arthur Balfour's *Defence of Philosophic Doubt* and acquired for a lifetime the conviction that no one has ever succeeded in constructing a logic-proof system of monistic metaphysics.

President Taylor's course in psychology, required in the first semester of all seniors, was based on James Clark Murray's *Handbook of Psychology* and lectures on the history of philosophy by Dr. Taylor. Murray's book was directed against the associational school, Dr. Taylor's lectures against materialism. Murray's argument was that association could not explain the process of active relating, which he called comparison: "association can merely associate." This was a sound position: James had expressed the same thing the year before in pointing out the neglect of 'selective attention' by the associational school. The problem is focal in psychology at the present time, with the believers in 'creative mind,' vitalism, voluntarism, and so forth on one side and the mechanists on the other: I firmly believe that it can be solved by mechanism, but not that of the old associative type. Dr. Taylor's attacks on materialism were made, not from the idealistic point of view, but from that of 'natural realism,' originating with Reid and still being defended in those days by President McCosh of Princeton. Dr. Taylor (whom, by the way, we regarded with great affection) had no idea of presenting metaphysical systems to us impartially: he wished to preserve our religious convictions by saving us from materialism in the one direction and pantheistic idealism in the other. This vigorous special pleading was more stimulating than the most conscientiously impartial presentation of opposing views could have been.

At the end of my senior year I had two dominant intellectual interests, science and philosophy. They seemed to be combined in what I heard of the wonderful new science of experimental psychology. Learning of the psychological laboratory just established at Columbia by Dr. Cattell, who had come a year before from the fountain-head, the Leipzig laboratory, I determined to be his pupil, and my parents took a house in New York for the year. But Columbia had never admitted a woman graduate student: the most I could hope for was to be tolerated as a 'hearer,' and even that would not be possible until after Christmas when the trustees had met. I solaced myself by taking the School of Mines course in quantitative chemical analysis at the Barnard laboratory, the second floor of a brownstone house on Madison Avenue. President Butler was then the amazingly efficient young dean of the department of philosophy, and, at his suggestion, I read Wundt's long article on psychological methods in the first volume of *Philosophische Studien;* having had only a year of German, I began by writing out a translation of it, an excellent way of getting the vocabulary. After Christmas I was allowed to present myself to Dr. Cattell for admission as a hearer. The psychological laboratory was the top floor of the old President's House on Forty-ninth Street close to the New York Central tracks. "What do you think is done in psychological laboratories?" asked Dr. Cattell, who looked then just as he does now, barring the grey hair. I blessed the hours I had spent on W. Wundt's article: instead of speaking as I am sure I was expected to do, of hypnotism, telepathy, and spiritism, I referred to reaction-time, complication experiments, and work on the limens and Weber's Law, and was rewarded by the remark that I seemed to have some knowledge of the matter.

From that time Dr. Cattell treated me as a regular student and required of me all that he required of the men. A lifelong champion of freedom and equality of opportunity, it would never have occurred to him to reject a woman student on account of her sex. The four men students, seniors, and I listened to his lectures, prepared reports on experimental work, and at least one paper on a theoretical subject. He assigned to me the experimental problem of finding whether Weber's Law held for the two-point threshold on the skin. I improvised apparatus, used a metronome to keep the duration of the stimuli constant, and found observers among my Barnard associates. Incidentally, it may be mentioned that Weber's Law does not hold for the two-point threshold. I also exchanged hours of observation with Harold Griffing, the only graduate student, who was engaged on his thesis, *On Sensations from Pressure and Impact.* He was a man of great promise, heavily burdened with the support of his invalid mother and sisters; he died a year or two after taking the doctorate. He would have been a leader in American psychology.

Dr. Cattell raised me to the height of joy after I had read a paper on the relation of psychology to physiology by writing me a note to suggest that I send it to the *Philosophical Review.* Nothing would have induced me to do anything so daring. At the end of the year, since there were no fellowships at Barnard, he advised me to apply for a graduate scholarship at the newly organized Sage School of Philosophy at Cornell. I feel an affectionate gratitude to him, as my first teacher, which in these later years I have courage to express; in earlier times I stood too much in awe of him.

While I was thus being initiated into Cattell's objective version of the Leipzig doctrine, the influence of William James's *Principles* was strong. His enthusiasm for the occult was unattractive; it seemed that in his zeal to keep an open mind he kept it open more widely to the abnormal than to the normal. But his description of the stream of consciousness, and the consistently analytic rather than synthetic point of view which he maintained in holding that simpler mental states are products of analysis, and in developing all spatial relations by analysis from a primitive space instead of compounding them like Wundt out of non-spatial elements, never lost their effect even though the prestige of the Leipzig school increased.

I went in the fall of 1892 to Cornell, where Titchener had just arrived from Oxford and Leipzig. He was twenty-five, but seemed older at first sight because of his square-cut beard; the illusion of age vanished on acquaintance. There was nothing about him at that time to suggest either his two greatest gifts or his chief failing in later life. The gifts, in my opinion, were his comprehensive scholarship, shown conspicuously in his *Instructor's Manuals of Experimental Psychology;* and his genius as a lecturer. In his first two years at Cornell his lectures were read, and were frankly after the German fashion: we regarded him as a brilliant young man who would give us the latest news from Leipzig, rather than as one to be heard for his own sake. The failing that later grew upon him was that of remaining isolated so far as his immediate surroundings were concerned from all but subordinates. In these first years he was entirely human. He once asked me to look over some proof; finding a sentence whose meaning was obviously inverted, I asked, "Didn't you mean" so-and-so? "Of course I did, ass that I am!" was the hearty response, a response that I fancy would have come far less heartily a few years later.

I was his only major graduate student, and experimental psychology was so young that he did not quite know what to do with me. Appointments for planning laboratory work would be made which often ended in his telling stories of Oxford life for an hour or two. He finally suggested that since I had some experience in work on tactual space perception, I should make an experimental study of the method of equivalents. I wrote up the results in a paper which was accepted in June at Vassar for an M.A. *in absentia,* Titchener having given me a written examination lasting three hours, of which I do not recall a single question. The paper was next year incorporated into my doctor's thesis, and was resurrected a few years ago by Gemelli in his study of the method.

The Sage School of Philosophy was an inspiring place to work, for the members of its faculty were nearly all young. I chose as my minor subjects philosophy and ethics. President Schurman taught the advanced course in ethics. He had visited Vassar in my senior year and given several lectures on Herbert Spencer, which it was my privilege to report as a college editor. They were models of clearness and force. I have always greatly admired him, and it is a keen pleasure to read of his diplomatic triumphs at an age when most men are resting on earlier laurels. To be his pupil was a privilege. I had also a course with Ernest Albee in Leibnitz, Hume, and Kant, and with William Hammond in Greek and mediaeval philosophy, and read Kuno Fischer with Frank Thilly one hour a week for drill in philosophical German. Among my fellow-students were Joseph Leighton, now of Ohio State University, Edgar Hinman of the University of Nebraska, Albert Ross Hill, later President of the University of Missouri, Melbourne S. Read of Colgate, and Louise Hannum, a remarkably able woman who afterwards taught in Colorado.

At the end of this year I was asked to take the Chair of Psychology at the Woman's College of Western Reserve University, and went to Cleveland to look it over. The opportunity was a good one, but I think I was wise in deciding to finish my work for the doctorate at Cornell, although Dr. Schurman disapproved of the decision. In my second year at Cornell I was no longer Titchener's only major student, being joined by Walter Pillsbury from Nebraska; this is an association of which I have always been proud. I had, during my work with the method of equivalents, thought of a subject for a doctor's thesis: the influence of visual imagery on judgments of tactual distance and direction. Much of my time this year went to the thesis. I had also a course in Lotze's metaphysics with F. C. S. Schiller, who had come from Oxford for a year's stay in the wilderness and was even then a very distinguished man. The thesis was finished by the spring vacation, and Dr. Titchener sent it to Wundt, who had it translated into German and published in *Philosophische Studien,* where the Leipzig theses appeared. On this occasion my translator enriched the German language with a new verb: *visualisiren.*

Examinations for the doctorate at that time were wholly oral. Some of the questions at mine I can recall. Dr. Schurman quizzed me on Spencer's *Data of Ethics,* which was a piece of luck for me, since in my senior year I had mildly annoyed Dr. Taylor and secured intervals of repose for my classmates by quoting it extensively. Dr. Titchener asked me something about Müller and Schumann's work on lifted weights, and also a question which I could not answer: the correct answer would have been, "The cornea," but why the cornea I have no recollection. Dr. Creighton asked me to name the Kantian categories, and what the relation of the third one in each group was to the other two; also about Berkeley's theory of causation. Dr. Hammond wished to hear about philosophy in the ninth century. The occasion was a pleasant one. I received the doctor's degree in June, 1894.

No position was waiting for me, and I even considered teaching psychology in a New York finishing school. The elderly gentleman at its head impressed me with its high standards: all the members of his senior class in astronomy the last year had attained the mark of 100 per cent. Before I committed myself to this institution a telegram asked me to come to Wells College. Its new president, Dr. William E. Waters, being a classical scholar, preferred to teach Greek instead of the psychology and ethics required of a college president; in this emergency they could offer me little money, but I gladly accepted the Chair of Psychology, Philosophy, and Ethics (not to mention logic), at a salary of three hundred dollars and home. (The arrangement with my family was that when I visited them they paid the expenses of importation, but I must pay my own way back). Wells was, and I hear still is, though much grown, a delightful place; I spent six years there that left not a single unpleasant memory. The salary, by the way, had by the last two years reached the maximum for women professors, seven hundred dollars and home; the men were paid fifteen hundred. What money meant in those days is shown by the fact that at the end of the six years I had saved five hundred dollars without any effort at all. During several of these years I spent one day a week at Cornell. Titchener was already withdrawing from contact on equal terms with his colleagues in the Sage School, who went their own way, and, as they were my especial friends, I saw little of the Director of the Laboratory, though he was always kind and helpful when we met. I published during this time some observations on after-images and two or three articles on other subjects which may remain forgotten. Late in the summer of 1897 I fell ill with typhoid during a visit in Vermont, and could not return to work until the first of December. Without my knowledge, Professors Creighton and James Seth had taken charge of my courses, coming down twice a week from Ithaca; thus my misfortune was great gain for my students.

During this period I accepted the general point of view of what Titchener called structural psychology. To a person with a liking for chemistry the idea of introspectively analyzing mental states into irreducible elements had attraction, yet one could not forget James's conception of consciousness as a stream and the impossibility that it should be at once a stream and a mosaic. I never followed Titchener when he developed his elaborate, highly refined introspective analysis, and not one of the doctor's theses produced at Cornell and later at Clark (under Baird) by the use of this method had any real appeal for me. It is worth while to describe conscious states, but not, in describing them, to turn them into something unrecognizable. Münsterberg's work was attracting attention. I liked the theory of knowledge which he developed in his *Grundzüge,* that brilliant book which he never finished, preferring to waste his great powers in writing articles for American popular magazines. He was a dualist but not an interactionist, a position which perfectly suited my own skepticism with regard to monism. He restricted causality to the series of physical events, and regarded psychic processes as epiphenomena; this still seems to me the only defensible position. His own method of structural analysis, however, which sought a psychic Ur-element as the accompaniment of the activity of a single cortical neuron, was indefensible psychologically, physiologically, and psychophysically; that is, his introspective analysis was fallacious; his physiological hypothesis that single cortical neurons act alone was highly improbable, and there was not the slightest evidence that his mental Ur-element was the accompaniment of such a physiological Ur-element! But his emphasis on movement as an explanatory concept seemed to me highly promising. Structural psychology was weak on the explanatory side: motor processes could help it out.

In my sixth year at Wells I became restless, and felt that a year at the Harvard laboratory would be a refreshing change. I was granted leave of absence for this purpose in the spring of 1900, but a telegram from President Schurman changed my plan. He asked me to come to Cornell as Warden of Sage College, with plenty of opportunity for my own psychological work and what then seemed the enormous salary of fifteen hundred dollars and home. So I returned to Cornell for two years. In the first, I tried to work out in the physics laboratory the problem of the flight of colors, but did not succeed in obtaining good results from any controllable source of light. I had to spend too much time and energy at social functions, which, however, gave much profitable experience in other directions. In the physics laboratory I served as an observer for Frank Allen's research on the fusion rate of retinal impressions from different regions of the spectrum, and got a further glimpse of the futility of elaborate introspection. As I observed and reported on the visual phenomena, I accompanied my judgments of fusion by introspective accounts of variations in my general state of mind which would undoubtedly make the curves in one experiment quite different from those in another. I mentally congratulated Mr. Allen in having for the first time an observer skilled in introspecting sources of error. Much surprise resulted when the curves proved highly uniform; the sources of error had not influenced the sensory judgments at all.

Moreover, I was now impressed with the lack of agreement among structural system-makers in regard to conscious elements and their attributes. I wrote a paper for Professor Titchener's seminar, which I occasionally attended in 1900-1901, comparing the structural systems of Wundt, Ebbinghaus, and Münsterberg, and showing how completely they were at variance in their conceptions of element and attribute. Titchener reacted strongly against this paper: I cannot remember what his specific objections were, but I realized the awe in which he was now held by his students when a member of the class came to me next day with sympathy for his 'injustice,' which no one had ventured to point out the night before! It had not occurred to me to be depressed by Titchener's criticisms; it was exciting to 'draw blood' from him. The paper appeared in Volume XI of the *Philosophical Review,* and I later found that it was welcomed by some of the rising school of functional psychologists as helping to demonstrate the uselessness of structural psychology.

In the second year of this later sojourn at Cornell I was appointed, with Titchener's very kind and cordial consent, a lecturer in psychology, and gave a course in social psychology based to some extent on Wundt's *Völkerpsychologie* (the volumes on speech were the only ones published at that time); and a course in animal psychology. My colleagues in the Department were Bentley, Whipple, and Baird, and one of my pupils was Robert Gault.

I wrote in this year another paper which indicated departure from Titchenerian doctrine; it tried to show the impossibility of regarding duration as an attribute of sensations.

Since this dissatisfaction with extreme structuralism was accompanied by a growing interest in motor processes, it might seem that functional psychology, now coming to the fore, would have been a refuge. Several influences kept me out of it. For one thing, it was the child of pragmatism, which was itself sponsored by Dewey and James. Professor Dewey is undoubtedly one of the few great leaders of American thought, and I have felt the charm of his character and personality, but, through some congenital disability, I cannot read him. And James, despite the enduring influence of his psychology, as a philosopher inspired me with distrust. The doctrine that ideas are true in proportion as they 'work' may too readily be used to mean that they are true in proportion as they are comfortable. Moreover, as Bentley pointed out in a review of Angell, after abandoning the description of conscious processes, functional psychology had little left to say except to show how they served 'the welfare of the organism.' Also, if it really meant that mental processes *as such* had significance for bodily welfare, this was interactionism, and I could not and cannot tolerate it.

Being a 'warden' and having to concern oneself with the behavior of other people was highly uncongenial, and when, at the end of two years, I was offered an assistant professorship in full charge of psychology at the University of Cincinnati, where Dr. Howard Ayers was President, I eagerly accepted it, though I disliked going so far from my parents. I was the only woman of professorial rank on the faculty, and President Ayers took especial pains to treat me, as we sat around a long table at faculty meetings, on a footing of perfect equality with the men. Judd had just resigned the charge of this laboratory to go to Chicago. My colleagues were an exceptionally able group, including, for example, Michael F. Guyer in biology, Louis T. More in physics, and F. Hicks, later President of the University, in economics. A drawback at Cincinnati was the quality of the student material. The University was compelled to admit all graduates of city high schools, and at the end of my first term I had to condition half my introductory class. The place offered, however, many opportunities, but it is hard for a deeply rooted Easterner to be transplanted. When I sat in the station and heard the train called for "Buffalo-Rochester-Syracuse-Albany," the sound was sweet in my ears, and I can still remember the thrill of happiness that came with the first stir of the car wheels on their eastward journey. I was thankful when President Taylor in the spring of

1903 called me to Vassar as Associate Professor of Philosophy. There I could spend every Sunday with my parents, who were living only sixteen miles away.

Psychology at Vassar since Dr. Taylor gave it up had been taught by the Professor of Philosophy, H. Heath Bawden, a pupil of Dewey's and an inspiring teacher, who had made considerable reputation as a member of the pragmatist group. He had established the beginnings of a psychological laboratory in the basement of the building dedicated to biology. All the work in psychology was turned over to me, with the addition of a semester's course in ancient philosophy and one in modern philosophy. No one will care to read an account of the progress of courses in psychology at Vassar: it is enough to note that an independent department was formed in 1908 and that the laboratory now occupies more than half of a three-story and basement building.

In order to give the senior students in psychology a glimpse of research methods, a few simple experimental problems were devised each year, whose results, if they worked out successfully, appeared in the *American Journal of Psychology* as "Studies from the Psychological Laboratory of Vassar College." The problem and method of a study having been determined by me, the experimenting was done by the students, who also formulated the results; the interpretation and writing of the reports fell to me and the paper was published under our joint names. Fifty-seven such studies have appeared. Only three times have we conferred the master's degree in psychology; the college does not accept candidates for the doctorate. Personally, I deprecate graduate study for women at any but coeducational universities.

In my first year at Vassar (1903-1904) I wrote a short article developing the idea that both the capacity of making sensory discriminations and that of recalling memory ideas have been dependent on the possibility of delaying reaction, a possibility which arose from the development of distance receptors. Two years later the importance of these receptors for mental development was pointed out by Sherrington in *The Integrative Action of the Nervous System.* In 1904 I read before the Psychological Association a paper showing some of the difficulties involved in the Wundtian tri-dimensional theory of feeling, and expounding the idea that feeling is the unanalyzed remainder out of which sensations have emerged; the power of analysis having been earliest developed where it was most needed, that is, with reference to outside stimuli. The genetic point of view was much in my mind during these years, and so were kinaesthetic processes. To the Stanley Hall *Festschrift* in 1903 I contributed the suggestion that the social reference of certain conscious states, e.g., the thought of another's suffering, had as its nervous basis kinaesthetic processes from certain incipient reactions, for instance the impulse to help. At a symposium on the term 'feeling,' held at the 1905 meeting of the Association, I defined feeling as the unanalyzed and unlocalized portion of experience, and suggested that James's feelings of 'but' and 'if' might be the remnants of ancestral attitudes. James was present and approved of the idea.

If this were an emotional instead of an intellectual autobiography, an almost morbidly intense love of animals would have to be traced to its occult sources. Animal psychology began to occupy me when I gave a course on it at Cornell. During a six-weeks' stay at Ithaca in the summer of 1905, I collaborated with Dr. Bentley in some experiments on color vision in a brook fish which he captured from a neighboring stream. The chub learned with great speed, in spite of lacking a cortex; it discriminated both dark and light red from green. Our method of eliminating the brightness error by varying the brightness of the red was inadequate, but later investigators have confirmed our results. Shortly after this, I began to collect and organize literature on animal behavior. The Animal Behavior Series, which the Macmillans published under Dr. Yerkes' editorship, brought out the first edition of *The Animal Mind* in 1908. While the objective school of interpretation, represented in America chiefly by Loeb, had long urged that much animal conduct should be regarded as unaccompanied by mind, no one had then suggested that all animal behavior, still less that all human behavior, is unconscious, and the patterns of animal consciousness seemed to me then, as they do now, well worth investigating and perfectly open to investigation.

The experimental study of thought processes that was now being carried on by the so-called Würzburg School suggested a further use for kinaesthetic explanations, and in 1909 I wrote a paper on "The Physiological Basis of Relational Processes" which developed more fully the idea propounded at the 'feeling' symposium, and maintained that relational processes in general are close fusions of kinaesthetic excitations. The arguments for this were that kinaesthesis accompanies all other sensory experience and is a suitable basis for relational processes, which are common factors in different sensory situations; that many relational processes are for introspection accompanied by kinaesthesis from attitudes; and that where the relational processes are unanalyzable into kinaesthetic sensations, this may be due to the fact that the attitudes concerned are phylogenetically very old, and that there has been little practical need for such analysis—the point made in 1904.

The influence of the Würzburg School and of Ach also brought to the fore in the psychology of the time the problem of purpose, the *Aufgabe*. Structural psychology had overlooked this in its strong bent towards reducing everything to sensations. I was enough of a structural psychologist to seek for sensational explanations if haply they might be found, for the reason expressed in the 1909 paper just mentioned: "The sensory process is one about which we know more, it is of a less conjectural and made-to-order character, than other hypothetical cortical processes." That the basis of an *Aufgabe* should lie in an attitude seemed especially possible, since the essence of a purpose lies in its tendency to persist, and attitudes are characteristically persistent as compared with movements and with sensory processes. At the 1913 meeting of the Association I presented the following ideas: "An essential characteristic of an *Aufgabe* . . . is that it associates with itself a bodily attitude which may be called the activity attitude. The *Aufgabe* may drop out of consciousness and still influence associative processes if the organic-kinesthetic fusion resulting from the attitude remains." This doctrine of a persistent bodily attitude as the basis of purposive thought and action has strengthened its hold on my mind since its first formulation.

In the next year, the fundamental principles of a motor system of psychology were laid down in an article entitled *The Function of Incipient Motor Processes.* It explained association as essentially the association of movements, a doctrine based on the fact that impressions do not become associated merely by occurring together, but only if they are attended to together, attention being regarded as a motor process. It also presented a physiological theory of the image or centrally excited sensation. "If a motor response is initiated" and antagonistic excitations delay its full performance, "all the sensory centers that have recently or frequently discharged into the motor center concerned . . . are set into excitation," this process being accompanied in consciousness by imagery. The idea of incipient or tentative movements had been vaguely in my mind since 1903, when in the Hall *Festschrift* article I made use of "sensations resulting from the stirring of an impulse."

During all those years most of my vacation time was spent with my parents. The family fortunes having declined, they were living at Newburgh, enjoying a superb view of the Hudson but little variety, and I was disinclined to leave them for long. In the summer of 1906, while working on *The Animal Mind,* I spent three weeks at Cambridge looking up material in the Museum Library, and had an opportunity to see Professors James and Royce at their homes. From 1913 to 1917 I taught in the Columbia Summer School and with great pleasure and profit came to know Woodworth, the Hollingworths, Poffenberger, and the Montagues. I occupied Dr. Cattell's office, opening out of Dr. Woodworth's, and admired the chances of fortune that had raised me so high. In December, 1914, my father died and my mother came to live with me at Vassar until her death in 1924. Both my parents always took pleasure in my work.

I had for some time been collecting the results of all the German and French experiments on the higher mental processes. Vassar celebrated in 1915 the fiftieth anniversary of its founding, and the trustees decided to publish a commemorative series of volumes by alumnae, books of a scholarly rather than popular nature, which might not readily find a publisher in the ordinary way. For this series I wrote *Movement and Mental Imagery,* trying to interpret the experimentally obtained data on the higher mental processes by the motor principles I had been evolving, and developing the doctrine that thinking involves tentative or incipient movements. Since the series was published in so uncommercial a fashion, it got little advertising, but at least I can say that '*M. and M. I.*' has considerably increased its sales within the past three or four years. I shall never cease to be pleased that Hollingworth read it when it came out and spoke kindly of it, not to me but to his wife: a second-hand compliment has double value. Another person whose praise of it will always be remembered is Professor T. H. Pear, who reviewed it for the *British Journal* and discussed it in his *Remembering and Forgetting.*

In 1917, I wrote for Titchener's *Festschrift* an article which had nothing to do with the motor theory, but presented some ideas on which I had been basing a course in social psychology ever since 1902. The concept of *ejective consciousness* had proved itself useful in analyzing and classifying the phenomena of social relations. The term was borrowed from W. K. Clifford's 'eject,' by which he meant a state in another person's mind, and ejective consciousness was used to designate awareness of processes in other minds. Thus, while both man and lower animals act socially, man has a much greater tendency to think about what is going on in the minds of his fellow-beings than the animals have, and this tendency brings about characteristic modifications in social behavior. The term is not a motor one, but in the Hall *Festschrift* fourteen years earlier I had suggested a motor basis for ejective consciousness. In social psychology it seems more convenient to use the concept without discussing its physiology. It is serviceable in discussions of language, religion, and art, and is still employed in my social psychology course.

Watson's radical behaviorism was of course the favorite topic of discussion in the years from 1915 to 1922 or thereabouts. It will be remembered that his first attack on the existence of conscious processes consisted in denying that of mental imagery. A critic could easily point out that his principles required also denial of the existence of all sensation qualities. In fact, the existence of sensation qualities is irreconcilable with any materialistic monism. My presidential address before the 1921 meeting of the Psychological Association tried, while rejecting the Watsonian metaphysics, to show that introspection itself is an objective method and one necessarily used by the behaviorist.

The evening of that address was one of perfect happiness for the speaker, whatever the sufferings of the audience may have been. The scene was the beautiful Gothic dining-hall of the Princeton Graduate School, and I sat at the high table on the dais. At intervals during the banquet strains from the fine organ, under the skillful hands of Dr. Carroll Pratt, rose to the carved beams of the roof. I had rewritten my speech so many times that it was as good as I could make it and was dismissed from my thoughts. Just before we sat down, Dr. Walter Bingham had completely surprised me by saying that before my address he would announce the award of the prize of five hundred dollars offered by the Edison Phonograph Company for the best research on the effects of music, to a study made by my colleague Dr. George Dickinson of the Vassar Department of Music and myself on "The Emotional Effects of Instrumental Music." By the way, Dr. Bingham's memory played him false when, in his introduction to the volume, *The Effects of Music,* which contained the papers submitted in the contest, he stated that the prize was given to the other study which I had submitted, in collaboration with Misses Mead and Child.

At this meeting of the Association a Committee on the Relation of the Association to Publication was appointed, consisting of Messrs. Langfeld, Franz, and myself as Chairman. A previous committee under the chairmanship of Judd had been formed in 1920 and had been unable to present a report. The subject in hand was the development of a Journal of Psychological Abstracts, and the difficulty was as follows. Professor Warren, the owner of the *Psychological Review* publications, was conducting an abstract journal in the form of certain numbers of the *Psychological Bulletin*. There was a desire on the part of many psychologists that such a journal should be enlarged, which could not be done without a considerable subsidy; but no subsidy could be obtained, for example, through the National Research Council, so long as the journal was privately owned. At the 1922 meeting of the Association, our Committee reported that Professor Warren had offered to give the Association an option to buy the fifty-five shares of the Psychological Review Company's shares at fifty dollars per share, *plus* the accumulated unpaid dividends since the incorporation of the company, amounting to 42 per cent of the purchase price. Thus the Association would become the owner of all the publications. The Association authorized our Committee to see that the option was drawn up. On December first, 1923, Dr. Cattell, Chairman of the National Research Council's Committee on Psychological Abstracts, asked Mr. Langfeld and me to meet his Committee; we accordingly did so, and reported to the 1923 meeting that the Psychological Division of the National Research Council would try to obtain funds for the establishment of an independent Abstract Journal if the Association would vote to take up the option for the purchase of the *Psychological Review* journals and appoint a committee on ways and means of paying for them. The Association did so, appointing our Committee to continue in this function. In the spring of 1924 Mr. Langfeld and I met with Messrs. Anderson and Fernberger, the Association's Secretary and Treasurer, and a plan was formed which involved gradually raising the annual dues to ten dollars and paying off the debt by notes falling due in successive years. Meantime, Professor Warren had generously waived the matter of the unpaid dividends. This plan was laid before the members of the Association by mail for an expression of opinion, which was favorable by a large majority, and at the 1924 meeting the project was adopted. Meantime, I had been appointed by our Division of the National Research Council chairman of a sub-committee to secure a subsidy for the projected Abstract Journal, the other member being Professor Stratton. We met with a committee from the Association, consisting of Professors Langfeld, Fernberger, and Hunter, and made out a budget, with Dr. Cattell's advice, for the new journal, requiring a subsidy of $76,500, which was subsequently obtained from the Laura Spelman Foundation. At the 1928 meeting of the Association, Professor Warren cancelled the remainder of the Association's debt to him.

By the way, the unreliability of having the Association express its opinion by mail was comically shown at this meeting. A project for the certification of consulting psychologists which I reported as having been favored by a considerable majority of the Association when presented through the mails was turned down vociferously by the psychologists present in the flesh. The mail canvass is, however, valuable for the publicity it gives a project.

During the next five years I attempted, with the aid of my students, various small studies in the difficult field of emotions; also, with the help of a grant from the American Association for the Advancement of Science, a questionary study on sources of pleasure, anger, and fear in groups of Italian and Russian Jewish women in New York and Chicago.

When the Division of Anthropology and Psychology of the National Research Council was constituted I was a member for the first year, but was not re-elected; in 1924, I was elected by the Psychological Association for a three-year period. While Dr. Stratton was chairman of our Division of the National Research Council, he established a committee on the experimental investigation of emotion, appointing me chairman and Drs. Dodge and Dunlap as the other members. I do not think my fellow-members were hopeful as to the prospects of this committee, whose object was to discover ways and means, including funds, for such investigation. In fact, when I arrived at our first committee meeting I found one of them on the point of resigning. He consented to remain, and we called a conference of experimental workers in the field of emotion at Columbia on October 15 and 16, 1926. It was attended by Messrs. F. H. Allport, Blatz, Brunswick, Dodge, Dunlap, Gesell, Landis, Moore, Nafe, Wells, and Woodworth, besides several others who came in for an hour or two. Our committee presented to the next meeting of the Council a report recommending the support of researches by Lashley and Landis. Unfortunately, at this time the Laura Spelman Foundation, the chief source of funds for our Division of the National Research Council, announced its intention of supporting no more individual projects, so the pessimism of my fellow-members was justified.

In the autumn of 1925 Titchener resigned the editorship of the *American Journal of Psychology* on account of a difference of opinion with Dallenbach, its sole owner, with regard to the ultimate disposal of the property. Dr. Titchener was, I am convinced, in the wrong, and the breach was a grief to Dr. Dallenbach, who had felt for him a most loyal affection. At the Christmas meeting of the Association that year, Bentley, Boring, Dallenbach, and I discussed in the study on the top floor of the Dallenbach house until midnight the future of the *Journal*. When we descended to the wives of these gentlemen, patiently waiting before the living-room fire, we had agreed to edit the *Journal* jointly, and thus began an association which has been unclouded by a single disagreement or unpleasant feeling. Dr. Dallenbach bears with conspicuous ability the heaviest burden of the work.

A second edition of *The Animal Mind* had appeared in 1917, nine years after the first, and a third one seemed to be due in 1926; this time the book was very considerably rewritten. Reviewing Woodworth's *Dynamic Psychology* in 1918, I began to realize how completely my motor theory had ignored the explanatory function of the *drive*. Of course, one had taken for granted that an animal would not learn without a motive, but, as I analyzed in 1926 the recent literature on learning, especially the work of Szymanski, it became clear that the drive explains the formation of successive movement systems by being present throughout the series, and by setting in readiness its own consummatory movements. A paper on "Emotion and Thought," written for the Wittenberg Conference on Feelings and Emotions, discussed some relations between the passage of drive energy into visceral and non-adaptive muscular movements, as in emotion, and into tentative movements and the 'activity attitude,' as in thinking. In my address as retiring Chairman of Section I of the American Association for the Advancement of Science, December, 1927, I used the passage of a drive into the activity attitude as a mechanistic explanation of purposive action, and urged that vitalism and emergent evolution, in general, are too ready to adopt the primitive mind's recourse to unknown forces. The address also suggested that a precursor of the activity attitude might be the 'orientation towards a goal' observed in animals learning a maze path; this idea was further developed and some experimental results, showing the influence on maze orientation of the presence of food during the running and of the initial run's direction, were presented at the meeting of the Ninth International Congress in September, 1929.

The enthusiasm with which the *Gestalt* psychology was being preached in America during these years by Köhler and Koffka was far from being unwelcome; it was a real pleasure to have the patterns of consciousness, surely among the most fascinating objects in the universe, made the subject of thorough study and experiment instead of being stupidly ignored after the behaviorist fashion. It did, however, appear that configurationism was inclined to take vocabulary for description and description for explanation, and might well be supplemented by motor principles. At the 1925 meeting of the Psychological Association I suggested how the nature of the motor response could be used to explain certain phenomena of perception which are fundamental in the *Gestalt* doctrine, and, in a round-table discussion at the International Congress meeting in 1929, I made a similar suggestion in regard to association. Köhler, in replying, said among other things, "Why should we be expected to explain? Why is it not enough for the present to describe?" or words to that effect. This delighted me, for I had expected him to say, "We configurationists have a thoroughly adequate principle of explanation, but unfortunately Miss Washburn is unable to understand it!" Which would have been unanswerable, because, in its latter portion, quite true.

The results of experimental work, if it is successful at all, bring more lasting satisfaction than the development of theories. Some of the small studies from the Vassar laboratory which have covered a period of twenty-five years do give me a measure of such satisfaction, to wit: certain observations on the changes occurring in printed words under long fixation; the fact that the movements of the left hand are better recalled than those of the right, probably because they are less automatized; the fact that movement on the skin can be perceived when its direction cannot; observations on the perception of the direction in which sources of sound are moving; observations on retinal rivalry in after-images; a study of the trustworthiness of various complex indicators in the free association method; experiments on the affective value of articulate sounds and its sources; the concept of affective sensitiveness or the tendency to feel extreme degrees of pleasantness and unpleasantness, and the fact that it appears to be greater in poets than in scientific students; the first experiments on affective contrast; the fact that the law of distributed repetitions holds for the learning of series of hand-movements; the study of revived emotions. In 1912, Miss Abbott and I proved red color-blindness in the rabbit, and, incidentally, that the animal reacts to the relative rather than the absolute brightness of colors, a fact later exploited by the configurationists. A student, Edwina Kittredge, proved that a bull-calf also was red color-blind; this coincided in time with Stratton's disproof of the notion that red angers bulls. In 1926, I published a study on white mice, in which, measuring activity by the actual speed of motion in the maze, and hunger by the time spent in eating, the effects of hunger and those of the impulse to activity were separated; another feature of this study was that each mouse's results were treated individually.

The Wittenburg Conference on Feelings and Emotions, October 19-23, 1927, was a remarkable affair. The readers of this article all probably remember how the wonderful energy and efficiency of Dr. Martin Reymert, with the enlightened support of the college administration, made the opening of a psychological and chemical laboratory at a comparatively small Ohio college a truly international event. It is likely that other speakers beside myself arrived at Springfield wondering whether any one but ourselves would be present to hear us. Dr. Carr told me while we waited in the crowd outside the assembly room that he had telegraphed Dr. Yoakum not to make the long journey up from Texas, as the conference might not justify the trouble; the latter disregarded this advice and came. In fact, the conference was worth far more than any meeting of the Psychological Association, since many of the leading psychologists of Europe and America sent or presented papers, and the audiences must have averaged five hundred psychologists. I had an odd experience at the close of the discussion of my paper. A fiery black-haired member of Congress startled the audience by rising from the fronts seats and fiercely challenging something I had been quoted as saying in a newspaper interview the previous day about the superiority of education over legislation as a means of reform. He reminded one so vividly of a statesman out of *Martin Chuzzlewit,* and the size of the disturbance he made was so comically out of proportion to the insignificance of its cause that one could not help enjoying the incident. Later I was told that he was seeking re-election. On the last day of the conference honorary degrees were conferred upon some of the chemists and psychologists; my being included was, I was sure, due to my having been the only woman speaker, but I liked being photographed standing between Dr. Cattell and Dr. Cannon.

The Christmas holidays of this same year were also full of excitement. A year previously Dr. Dallenbach had written me of the *Journal's* plan to publish a volume in my honor commemorating the end of a third of a century of psychological work. The project was to have been a secret until carried out, but "Boring's Quaker conscience" felt that I should be warned. I was quite overwhelmed at the prospect.

The Christmas meeting of the Association in 1927 was at Columbus, Ohio. As I was not well, and had to go to Memphis to give the address of the retiring Chairman of Section I of the American Association for the Advancement of Science, I decided to omit the Psychological Association meeting; but on learning that my *Journal* colleagues planned a dinner for me at Columbus, my duty and inclination were alike plain. On Wednesday evening I spoke at Memphis before a joint dinner for Sections H (education) and I; Dr. Haggerty, as the retiring Chairman of Section H making the other address. There were not more than forty persons at the dinner, and they were all educators, quite uninterested in what I had to say about purposive action; all the psychologists had left for Columbus except Mr. and Mrs. Gates, to whom I addressed myself with pleasure. The great advantage, however, which the section chairmen of the American Association for the Advancement of Science enjoy is that of having their addresses printed in *Science* and thus reaching the finest scientific audience in the world: I had later several interesting letters from men in other fields who shared the mechanistic point of view of the paper. At the close of the dinner I betook myself to the railroad station to wait from 9:30 P. M. to 3:30 A. M. for the only train that would get me to Columbus in time for the *Journal* dinner. The train was good enough not to be late, and, by dressing for dinner before reaching Columbus, I, too, was on time to dine with a group of friends in whose company I seemed very small and unworthy: twenty of the contributors to the Commemorative Volume (a title that made me feel like a blessed shade). Most of those who contributed were asked to do so because I had been associated with them on one or another of the psychological journals. Dr. Bentley presided, Dr. Pillsbury spoke for the *American Journal,* Dr. Yerkes for the *Journal of Comparative Psychology,* Dr. Langfeld for the *Psychological Review,* and my former colleague, Dr. Helen Mull, for Vassar, while Dr. Warren presented the volume in the wittiest speech of the evening. Dr. Dallenbach was the moving spirit of all.

A conference of experimental psychologists was called by Dr. Dunlap at Carlisle, Pennsylvania, in March, 1928, to consider ways of advancing experimental research: I did some preparatory work for this in analyzing the results of a questionary on the equipment of the various psychological laboratories which Dr. Dunlap had sent out in order to get a basis for choice of the institutions to be represented at the conference; and also the latest reports of the various foundations to find what percentages of their gifts had gone to pure science. Needless to say, the percentages were very small indeed. Of the several suggestions adopted at this conference, that of the formation of a National Institute of Psychology, with headquarters at Washington, is now being carried out. It reminds some of us of the lapse of years; active members of the Institute will automatically become associate members of the age of sixty. Another recent conference called under Dunlap's direction was of editors and publishers of psychological journals. Various matters were profitably considered; for example, an excellent set of rules for the preparation of manuscripts by authors was formulated. Unfortunately, it takes more time to make an author follow rules than to correct his manuscript oneself.

In the spring of 1928, having developed some fatigue symptoms, I took the only leave of absence I have ever had and went on a Western Mediterranean cruise, my first trip abroad since my fourteenth summer. Appetite thus being whetted, in the summer of 1929 I spent a fortnight in England.

The Ninth International Congress of Psychology, September 1-8, 1929, is still fresh in our memories; in mine it lingers as a recollection of talks with old and new friends, whether sitting on benches in the beautiful Harkness Quadrangle or at tables where we enjoyed the super-excellent food of the Yale cafeteria. I am sure our foreign friends will never forget the revelation of democracy in action which they obtained from standing in line and collecting their own sustenance at that cafeteria. I was elected to the International Committee at this meeting, an honor I appreciated the more because of the other Americans chosen at the same time.

One of the difficulties in writing these recollections has been that the present is so much more interesting than the past. It is hard to keep one's attention on reminiscence. Scientific psychology in America—though not, alas! in Germany, its birthplace—seems fuller of promise than ever before. The behaviorists have stimulated the development of objective methods, while configurationism is reasserting the importance of introspection; and, best of all, pure psychology is enlisting young men of excellent ability and a far sounder general scientific training than that possessed by any but a few of their predecessors.

ROBERT S. WOODWORTH

To begin with, it would seem appropriate for a psychologist called upon for his own story to treat his case as he would that of a problem child, by examining his antecedents and early environment with the object of revealing the causes that have made him what he is. Without attempting quite as much as that, I may at least disclose the fact that I grew up for the most part in New England and that all my ancestors for generations were New Englanders, though my genealogically minded relatives have never succeeded in tracing any of them back to the Mayflower. All the male ancestors seem to have been farmers, except for my father's father who was a school teacher, and for my father himself who was a Congregational minister and whose work took him to many churches in Connecticut, Massachusetts, Ohio, and Iowa. An ardent student of his Hebrew, Greek, and theology, he also read widely on other serious topics, and possessed a library that was awe-inspiring to me as a youngster, though I confess that I found little in it to read. Absorbed in his study and the weekly writing of his sermons, intensely and rather sternly religious, he permitted himself little relaxation with his children, except for afternoon drives about his country parish, when one or another of us was often delighted to accompany him. As I grew up during his mellowing later years, I became less and less afraid of him. He was aged fifty-five when I was born and died when I was twenty.

My mother was thirty-two when I was born, and she was my father's third wife. Her immediate family were successful farmers who took some part in public life in Massachusetts. She was one of the early graduates of Mount Holyoke Seminary (now College), and herself soon became the "founder" or first principal of a similar seminary in Ohio for young women, now Lake Erie College. She, then, was a teacher, and I might be said to have followed in her steps, since the subjects she taught included especially mathematics and "mental philosophy." I was the oldest of her three sons.

That so important question, where I came in the family, is not so easily answered, since I had four older half-brothers and sisters, two of whom were near enough my own age to be living at home, as young man and young woman, while I was a child. I can remember squabbling with this older sister, though not on fully equal terms. There was a more equal rivalry with the own brother three years younger than I. For five years, from the time I was twelve, while attending high school in a Boston suburb, I spent most of the time in the family of my oldest half-sister, who, with her husband, became like a second pair of parents to me, and whose three daughters were much like younger sisters. So you would probably diagnose me as an oldest child—Alfred Adler says it shows plainly in my "style of life." I was not free from timidity and feeling of inferiority, nor from a certain bumptiousness that broke forth at long intervals.

The "Oedipus complex," as far as I can discern, was represented in my case only by resistance to adult authority. Anything like mother-fixation does not ring true to me, thinking of my own childhood. My mother, while completely self-sacrificing and devoted to her children, was not sentimental or coddling. As far as I can remember, my attitude towards parents and older brothers and sisters was rather independent, though not exactly courageous.

To judge from my own case, recent emphasis on the "family situation," as all-important in the child's development, is overdone. My environment was the neighborhood rather than the home. In the Connecticut village where I lived from six to twelve years of age—after being born in Massachusetts and living most of the first six year in Iowa—and in the Boston suburb of my early 'teens, my competitions were with children outside the home more than with my sister, brothers, or nieces. The boys and girls I played with, the neighborhood bully who made me eat dirt, the men who would talk with me while doing their outdoor work, certainly deserve mention along with my own family as environmental factors. Our gang carried its playful, and sometimes only half-lawful, activities all over the village and out into the surrounding country, and the breaches of home discipline for which my mother had occasionally to snip my ears or my father to apply the birch consisted usually in my outstaying my leave when off with the gang. Always, from the age of six or seven, I had a chum, I had "a girl," I had a group of friends, whose doings loom larger in my memory than what went on within the four walls of home. So when I read case studies of children, in which the members of the family, along perhaps with the teachers, are made to appear as the only actors of importance, the picture seems unreal to me, or at least atypical.

Fortunately, however, I have not been asked to trace my development as a human being, but only as a psychologist. My earliest aspiration, as far as I know, was to be an astronomer. Later, at about the age of fourteen, I had very serious intentions of going back to the land and becoming a farmer—not so far "back" at that, since I lived in a rural community and was accustomed to some varieties of farm work. On graduating from the high school, I made a definite request of my parents to be allowed to attempt a career in music, of which I have always been very fond, but was easily persuaded to go on to college and delay decision on that matter. By the time I finished college, the music had dropped out of sight, except as an avocation, and I was committed to a scholarly career of some sort. Meanwhile, my parents' hope was all along that I should enter the ministry, but their pressure was very gentle, and when my own choice settled upon some form of teaching, there was no family opposition. In fact, I was so enthusiastic a student that my future seemed marked out for me.

But how and when did I come to fix upon psychology? That is a long story, and rather obscure. I remember meeting a word in my youthful reading which I pronounced "pizzicology," but I had no more idea what it meant than do many students today who elect a first course in our subject. Along through my teens, I was much of a Bible student, my interests being somewhat theoretical and quasi-theological; and I vaguely anticipated a study called philosophy, which should deal thoroughly with such matters, and in which I hoped to shine. I also remember Bacon's *Essays* as a favorite reading during those years, and I even wrote an essay or two of my own in the Baconian manner, seeking to set down wisdom in matters of the mind and of human conduct. I will quote a passage or two from Bacon to indicate my earliest models, and the sort of thing I hoped to do. One may say that already while in the preparatory school I aspired to be an armchair psychologist.

In glancing over Bacon's *Essays* just now, I recognize some passages which impressed me in those early days:

> "This communicating of a man's self to a friend works two contrary effects, for it redoubleth joys, and cutteth griefs in halfs; for there is no man that imparteth his joys to his friend but he enjoyeth the more, and no man that imparteth his griefs to his friend, but he grieveth the less."
>
> "Let not a man force a habit on himself with a perpetual

> continuance, but with some intermission, for both the pause reinforceth the new onset, and if a man that is not perfect be ever in practice, he shall as well practice his errors as his abilities, and induce one habit of both, and there is no means to help this but by seasonable intermission."
>
> "The invention of young men is more lively than that of old, and imaginations stream into their minds better, and, as it were, more divinely. . . . Young men are fitter to invent than to judge, fitter for execution than for counsel, and fitter for new projects than for settled business; for the experience of age, in things that fall within the compass of it, directeth them, but in new things abuseth them."
>
> "Reading maketh a full man, conference a ready man, and writing an exact man."

Meanwhile my actual study in the high school and well on into college was concentrated upon the classics and mathematics, with some history, a little modern literature, and very little science. At that time in Amherst College the philosophy course, which included psychology, was deferred to senior year. It was taught by Charles E. Garman, a splendid and remarkable man, regarded by nearly all his students as the best teacher they ever had. I looked forward to this course as the consummation of all things, and managed to secure an introduction to Garman during junior year. With his usual responsiveness to student needs, he inquired as to my preparation for philosophy, and was dismayed at the little science I had studied, for how, he asked, could I grasp philosophy without some acquaintance with scientific ways of thought. He advised me to do as much reading in science as I could during the coming summer vacation, and went to the college library with me to select a list of books which I might read by myself with some profit. There is no doubt that that interview was an eye-opener to me, and a turning point in my career, for thenceforth I regarded science as the general field of my efforts. Such sciences as I could still work into my college course I elected, but I regarded the philosophy course as the main thing.

This course, which extended through our senior year with an average of over six hours a week, started with psychology, and was called psychology throughout by the students. Garman used the psychology which he introduced rather as a means than as an end, choosing dramatic topics like hypnotism to catch the student's interest and lead into philosophical and ethical problems. In September, 1890, when I entered this course, James's *Principles* had not yet appeared, and there was probably no book in existence that would be recognized today as a textbook in psychology—not in English anyway. Our nearest approach to a psychological text was Carpenter's *Mental Physiology,* a book dating from about 1870, yet not so bad, as I see now on reexamination. Such topics as the *modus operandi* of sensation, perception, memory and imagination, aroused my interest, but the whole course, which gripped me with all force, was a continuous push towards the solution of fundamental philosophical problems. Garman insisted on our entertaining the most radical hypotheses, thinking them through, weighing the evidence, and coming to terms with each view before we passed on. Like Descartes, and in part with him, we passed through the valley of the shadow of universal doubt, and emerged with what we believed to be an indubitable positive philosophy, though I must admit that I was personally less sure of this positive philosophy than I was that somewhere in this field lay my work. Psychology and philosophy were not clearly distinguished in my mind. As a sample of the type of psychology that I then knew, let me quote a passage from one of the original pamphlets which Garman used with excellent effect in his effort to meet his students exactly on their own ground as he found it to lie from day to day.

> "Everywhere we are correcting and rearranging sense phenomena according to our code. What does not square with this we call illusion. . . . Then, again, as to the order of phenomena. Just keep a 'day-book' and record your mental pictures exactly as experienced and note the inextricable confusion. Here is a sample: Sitting in my study during a summer evening I am startled by a brilliant flash of lightning; item No. 1. Some one cries out in fear; No. 2. Doorbell rings. A book agent enters and insists on showing me a new atlas. Just as I am looking at the chart giving the ocean currents I hear a heavy clap of thunder; items 3, 4, and 5. Next the rain falls in torrents. My telephone rings and I talk with my friends who tell me that their house was struck. The railroad train whistles. Then comes a gust of wind that is a veritable hurricane. Conversation follows about the storm. Book agent presses his claims for further examination of maps and I am soon in China studying the position of Russia. Storm subsides—other flashes of lightning—telephone again rings—more thunder—other callers come. I retire and dream of China. Here are numerous items badly confused. In the morning I go to my classes. In the afternoon I take a drive and find a bridge up and a tree

> shattered. Here are a few phenomena, but there are a multitude that I have not recorded. No two days is there the same sequence, yet somehow all this confusion causes me no trouble, for from the 'day-book' I post a ledger and connect events not as they appeared but as they really happened. Then I make the lightning the antecedent, not of the coming of the book agent, but of the thunder and the riven tree. The loss of the bridge was the sequence, not of my drive, but of the storm the night before. Not in the day-book of sense, but in the ledger of common sense or judgment is there order" (Eliza Miner Garman: *Letters, lectures and addresses of Charles Edward Garman,* 1909, pp. 213-214).

On graduating from college in 1891, I thus had some acquaintance with a philosophical type of psychology, and a definite slant towards that subject. Yet it was twelve years more before I was definitely committed to a career in psychology. At the outset I was advised to teach for a while, rather than to continue to "absorb." I taught mathematics and science for two years in a secondary school, and mathematics for two years more in a college. During these last two years I devoted myself assiduously to mathematical study, and, when I broke off teaching to repair to a university, it took me several months to decide whether to continue in mathematics or to swing back to psychology.

But, during these four years of teaching, I had been subjected to two important influences towards psychology. These influences were William James and G. Stanley Hall. I possessed myself of James's *Principles* soon after its publication and was much stimulated by it. Hall's conception of a university as the home of untrammeled study and research had roused my enthusiasm as a senior in college and contributed towards my choice of an academic career; and some years later I heard him lecture and was much taken by his way of saying, "We now know," or "We are just finding out." I seemed to glimpse the frontier of scientific discovery, and, on returning to my room, I inscribed a card with the motto, INVESTIGATION, and suspended it by my desk. Though my "investigations" for the time being were mathematical and not psychological, the influence was felt a year later, when, on entering Harvard, I decided to quit mathematics for psychology and philosophy, the two not being clearly distinguished in my mind, any more than they were in the organization of the university.

My first two years at Harvard were divided almost equally between philosophy and psychology, and my principal teachers were James and Royce, to each of whom I was much devoted, while each of them was kindness itself to me. With James I studied general and abnormal psychology. Münsterberg was back in Germany for these two years, and I did not come into contact with him, but his place in the laboratory was taken by Delabarre, with Lough as assistant, and from them I had my first lessons in psychological experiment. Several of the subjects on which I worked and wrote for Royce and James have continued to interest me. The perception of time was one of these, on which, however, I have never published, though I have had students working on this problem. Another subject was "Thought and Language." I was challenged by the dictum of Max Müller, one of the folk-psychologists or philological psychologists, to the effect that there was no thought without language, and that the science of thought should be based upon the science of language. My own experience did not bear this out, since I often had difficulty in finding the words required to express my meaning, and since, in geometrical thinking, which had been one of my favorite pursuits, I was sure that I thought in terms of diagrams and gestures rather than in words. In fact, to think clearly in geometry I had to get away from words. Max Müller had said that counting would be a crucial case, and had asserted that counting could not be done except in words; but I found by experiment that I could count by rhythmical groupings and could group the groups and so work up to over 100, converting the rhythmical result afterward into ordinary numbers. I have returned to this subject a number of times, as in considering the curious discrepancy between colors as seen and colors as named, and again as incidental to the work on imageless thought; and recent attempts to revive and modernize the old theory that thinking consists in speaking have always found me skeptical, mostly because my own experience convinces me that there are other modes of thought besides the verbal, and that these other modes are more direct and incisive.

This study of thought and language was begun as a term paper in Royce's course on logic. James, in his abnormal psychology—a course in which he was at his best, and in which he became well known to his students through the visits to institutions on which he piloted us—James set me to work on dreams. Besides consulting the literature, I recorded many of my own dreams and made certain experiments on the speed of continued association and revery in waking conditions, as a check on the often asserted extraordinary speed of dreams. I found the speed sufficient in waking revery to account for all the instances of rapid dreaming that had seemed so remarkable. I also was led by my readings and records to a hypothesis on the cause of dreams that I have often wished I had published, as it has a certain resemblance, along with a difference, to Freud's conceptions which were published a few years later. Ives Delage had pointed out that we do not dream of matters that fully occupy us during the day, but of something else. I thought I could see that we dreamed about matters that had been opened up but interrupted or checked during the day. Any desire or interest aroused during the day, but prevented from reaching its goal, was likely to recur in dreams and be brought to some sort of conclusion that was satisfactory in the dream, while activities that had probably taken much more time and energy during the day, but had been carried through to completion, were conspicuous by their absence from the dream. But the wishes "fulfilled" in the dream, according to my idea, were of any sort—sometimes mere curiosity—and the suppression of them which had occurred during the day might be the result of external interruption as well as of moral censorship.

Other problems which took hold of me during those student days and which have continued to exercise me are those of motivation and of the mind-body relation. I remember saying to Thorndike, my fellow student, whose sane positivism was a very salutary influence for a somewhat speculative individual like myself, that I was going to try and develop "motivology"; and he agreed that it was worth doing. Always searching for some fruitful attack on this problem, I was naturally much interested in the works of Freud and McDougall a little later; and I have taken one or two shots at the problem myself, but have to agree that the desired science of motives is still very embryonic. As to the mind-body relation, it was not till some years later that I reached any solution that satisfied me.

I reached the end of my second year at Harvard without definite commitment to either philosophy or psychology. In philosophy, I had passed through a stage of absorption in the pantheism of India—the "That art thou" philosophy—but Bradley's *Appearance and Reality* had about convinced me of the relativity of all human modes of thought, so that no positive system of philosophy could claim any absolute validity. But still I was much interested in ethics and especially in logic—as taught by Santayana and by Royce—and was quite willing to continue working at them. But need for a decision arose when James secured for me the opportunity of a year in the physiological laboratory and recommended it strongly if I were going on with psychology. I consulted Royce on the matter, and he quite agreed that it might be better for me to choose psychology! And so, in 1897, I turned from philosophy, half expecting to continue some effort in it, but discovering, as time went on, that psychology was amply sufficient occupation, and that philosophy would be an undesirable distraction. The path of psychology at that time led between mountains, down through a valley that seemed to open out below into fertile country; but there were alluring trails up the mountains into which one was likely to stray with such satisfaction as to lose interest in the arable land below. Nowadays, psychology has emerged upon the plain, and the mountains are more distant and less enticing.

But the choice of psychology meant physiology in the first instance, and five of my next six years were spent in physiology, the final decision for psychology not being reached till the end of that time. The physiologists with whom I studied and taught were Bowditch and Porter of Harvard, Graham Lusk of New York, Schafer of Edinburgh, and Sherrington, then at Liverpool. My physiological studies were on the heart, stomach movements, carbohydrate metabolism, electrical conductivity of nerve, cerebral localization, and reflex action. Of my fellow students during this period, I specially remember Cannon at Harvard, and his early studies of stomach movement by aid of the X-rays, in the course of which he was led to study visceral processes as related to emotion and thus to establish an important link between physiology and psychology.

But meanwhile one of these years had been devoted strictly to psychology, and during that year at Columbia I was working with Cattell, whom I count as the chief of all my teachers in giving shape to my psychological thought and work. His emphasis on quantitative experiments of the objective type, and his interest in tests for individual differences, were powerful influences with me. During this same year I studied anthropometry and statistical methods with Boas, and gained from him and also from Farrand some appreciation of the value of anthropology to a psychologist. My experimental work during this year was on the control of muscular movement, a topic on which I continued to experiment at intervals for some years. Soon afterwards, while teaching physiology, I collaborated with Thorndike in studying the question of transfer of training, and this also is a subject to which I have returned again and again.

At the beginning of 1903, then, I was Sherrington's assistant at Liverpool, and much minded to make my psychology contribute to a career in brain physiology, rather than vice versa. Sherrington, to whom I owe very much, was willing that I should remain with him and develop my experimental psychology and brain physiology together. Just at this juncture, Cattell called me back to Columbia to work at experimental and physiological psychology, and careful consideration indicated that this was, after all, the line for which I was best prepared. Never was a finer chief than Cattell, alike in personal, departmental, and strictly scientific matters. So, fully twelve years from college graduation, after studying mathematics, philosophy, and physiology, I finally settled down to psychology as a member of the staff of Columbia University, and there I have steadily remained, aside from certain summers and leaves of absence. One of the latter, in 1912, I spent in Külpe's laboratory at Bonn. Though I was over forty years old at that time, I like to count Külpe among my fathers in psychology.

In returning from England in 1903, to enter the Columbia Psychological Department, I was fortunately able to bring with me a young wife, and we soon became part of a small group of congenial young couples in the University. For years we lived in the "Montrose colony" in the woods nearly forty miles up the river, with the Thorndikes, the Woodbridges, the Keppels, the Bagster-Collinses, and others later, and our four healthy, lively youngsters grew up in a group of twenty children. Though I never attempted any systematic psychological study of my children, I had my eyes psychologically open in watching them, and have certainly learned much from them. As they have grown up, I have not made any effort to steer them towards academic careers—in spite of my own great satisfaction with such a career—and they have tended towards the business field. For a period of years, I had considerable land to play with, and actually did get "back to the land" rather intensively, in the way of gardening, wood-chopping, and road-making, heartily enjoying this outdoor work and perhaps spending too much time on it. Always delighted with the woods, the mountains, the plains, the sea, I have been in later years quite an enthusiastic motorist, enjoying both the driving itself and the trips and scenery. Sometimes I have wished that I had gone into geology or anthropology, so as to have a professional excuse for faring afield. I have never succeeded in solving the problem of finding time for outdoor interests, family interests, musical interests, general reading interests, university, and departmental interests—all of which have been very genuine interests in my case—and still concentrating on what always remains my main interest, psychological research.

Even within the confines of psychology there is a wide field to wander in. My lectures have varied in topic from year to year. Aside from the general introductory course, in which I have taken a hand from time to time, my stand-bys have been experimental and physiological psychology. But for many years I lectured on abnormal psychology, and for another long series of years on social psychology. At one time or another I have lectured on tests, statistics, the "problems and methods" of psychology, its theory, history, and applications. Special topics on which I have repeatedly held seminars include movement, vision, memory, thinking, and my old hobby, motivation. Of late years, however, I have limited my courses to experimental psychology and to a survey of contemporary schools and debated questions.

From the beginning the research activities of the staff and students have centered in the "Seminar"—Cattell's Seminar, as it was at first. Here each candidate for the doctor's degree presented his research plans, his progress from time to time, and finally the outcome of his work, for consideration by fellow students as well as professors. Cattell's criticism could be keen as well as kindly. He was skeptical of any result that did not come out with a small "probable error," and with work which did not take account of what had been achieved by previous investigators. I remember one student whose seminar report seemed to indicate both sloppy work and poor perspective, and who disappeared altogether from our midst directly afterward. In the selection of dissertation topics, Cattell followed a plan which may have been derived, by antithesis, from that of his master, Wundt. Cattell expected the student to make the first move. The student was expected to have a problem on which he desired to experiment, and, if a workable plan of attack could be mapped out, he was told to go ahead and to depend largely on his own initiative.

I have followed Cattell in this respect, but the plan has certain disadvantages with a large group of students, many of whom desire above all things to get away from the "conventional" and, if possible, to discover something about "personality." I have given some sort of advice and guidance to students working on a great variety of problems. Of recent years, with a larger staff to divide the field, and with the attitude taken by the University (as represented especially by Dean Woodbridge) and by Poffenberger as executive head of the department, that each professor should have his own research interests to which the student must adjust himself, the scattering of effort has mostly disappeared.

But the scattering of effort has not been entirely the fault of the students. There seem to be many interesting problems in psychology, and from time to time new ones have been added to the list of my active interests. I have mentioned the thinking process, time perception, transfer of training, motor control, and motivation, as topics which interested me in my days of apprenticeship; and these have reappeared time and again in the work of my students. The study of motor control led over into an examination of the question as to whether or not kinaesthetic images were essential as the immediate antecedents of voluntary movements. In this study I departed from the custom of our group and used the introspective method, feeling half ashamed of myself for doing so; but the agreement of different subjects seemed to justify the method, and I continued to use it for an examination of images in perception and thinking, and thus was led into the "imageless thought" controversy. This was in 1906-1908.

I recently found in my files an old memorandum with the heading, "Hammering at the images," which projected several additional ways in which an attack could be made on the false prominence of the image in psychological theory. However futile the imageless thought discussion may now appear, it played a part in relegating images to the relatively minor position that they occupy in present psychological theory. I was far from doubting the existence of images, for I have abundant auditory images myself—of speech, of music, of noises—and I have not the least reason to question the testimony of psychological colleagues who speak with similar certainty of visual images in their own cases. The bald statement, sometimes heard of recent years, that images do not exist, strikes me as simply vaporing; while the statements that they are muscular movements, or sensations with present peripheral stimuli, are hypotheses worth entertaining, but far from established and with the balance of probability against them, in my opinion. At any rate, the processes that we have called images really occur in abundance; of that there can be no doubt; but the point of the imageless thought contention was, and is, that these imaginal processes are often almost if not quite absent just when thought is proceeding actively, and that, therefore, there must be thinking processes which are not imaginal processes. I still believe that this finding is genuine and of importance in dynamic and physiological psychology.

But the question of images in thinking is only a small part of the whole problem; and I like to believe that the series of studies of thinking that have issued from time to time from our laboratory have contributed bits towards the understanding of this fascinating performance. It is a rather elusive sort of performance, and, though introspection shows us much about it, the great need is to find objective methods for studying it in the laboratory. Promising leads are to be found in the memory experiment and in the transfer experiment. Memory can be aided by seeing relations in the material to be learned, as G. E. Müller has abundantly shown; and, consequently, it would seem, an *aided memory* experiment should afford an objective means of studying relational thinking. Again, problem solution, depending as it certainly does on the utilization of past experience, demands the *transfer* of what has been learned into the novel situation. It is partly for this reason that memory and transfer experiments have continued to appeal to me and have appeared frequently in the output of the laboratory.

Motivation has always seemed to me a field of study worthy to be placed alongside of performance. That is, we need to know not only what the individual can do and how he does it, but also what induces him to do one thing rather than another and to put so much energy into what he does. We need a study of motivation in order to understand the selectivity of behavior and its varying energy. In my books I have sought repeatedly for a formula that should bring motives right down into the midst of performance instead of leaving them to float in a transcendental sphere. The main object of such a formula, provisionally, is to free the conscience of the hardheaded experimentalist of any qualms he might otherwise feel in entering this subject. Here, again, a survey of the studies that have come out of our laboratory yields the comforting thought that, though I have not personally conducted many researches, I have probably played some part in an advisory capacity.

My interest in psychophysics was stimulated in the first instance by my master, Cattell. Certainly the psychophysical methods present themselves as a challenge for further inventiveness as well as for patient standardization. When I have assembled the results now available for generalization, I have been dismayed by their divergence of methods and consequent lack of comparability. At this point, psychology comes into much-to-be-desired relations with such advanced sciences as physics, chemistry, and astronomy, and should certainly be eager to do its share in bringing the subject into some kind of order. This interest also has borne some fruit in the research of the laboratory.

At intervals my old mathematical interest has re-asserted itself, and I have spent happy days endeavoring to work out some useful statistical device or in making statistical computations and graphs. With the accomplished mathematicians who are now marching in the psychological procession, I have naturally fallen far behind the band, but without losing the thrill of it.

At various times, from 1904 on, I have tried my hand at the devising and perfecting of tests, the chief work of this sort being the joint product of Wells and myself, the *Association Tests* of 1911. The "Psychoneurotic Inventory," or "Personal Data Sheet," was another effort. There have been many student researches in the field of tests that I have supervised more or less closely. Of late, in the division of labor within the Department, I have ceased to concern myself actively with tests, though I will admit that I still have in the back of my mind one or two schemes for tests that I should like to work out.

With all this sad array of scattered interests, I hope I shall receive credit for not dabbling to any appreciable extent in animal psychology, which is, in fact, a branch of psychology in whose general significance I most heartily believe and in which I should have liked to be myself a worker. The same can be said of child psychology. I have done what I could, as opportunity offered, to push forward these lines of research.

The story would not be complete without reference to activities that have taken me outside the University—and the University, it should be said, has been generous in lending its men to worthy scientific or public enterprises. The first such enterprise in which I took part was the World's Fair at St. Louis in 1904. Having provided for the assembling of representatives of many different races, the Fair also made provision for anthropometric and psychometric study of these samples, and I had direct charge of this work, with Frank G. Bruner for my chief assistant. We examined about eleven hundred individuals, making the standard physical measurements of the anthropologist, and also testing muscular strength, speed and accuracy, vision and hearing, and intelligence as well as we could with formboards and other simple performance tests that we devised. When the Fair was over, we promptly worked over our data, and reported some of the results at scientific meetings. Bruner published the results of the auditory tests as his dissertation, and I gave a general summary of our results and their bearing on the question of racial differences in mental traits. Further than that, the results have never been published, not from any doubt on our part as to their value, but partly because of the unlimited number of fascinating correlations which still remained to work out, partly because of the expense of publication, and partly, I am afraid, from a certain inertia or indifference to publication on my part. Once I have worked out the results, and perhaps reported them at a meeting, I feel satisfied.

There are a number of other studies which I have brought to some sort of conclusion but never published except in the reports of meetings where I have presented them. One such paper, read in 1905, demonstrated to my own satisfaction that vision during eye-movements was just about what would be expected from the retinal stimulation received and thus afforded no ground for assuming any special inhibitory effect of the eye-movement upon visual sensation; but I did not publish this paper, because I found that most of my confreres needed no elaborate convincing of this proposition. Another paper developed a statistical method of measuring rank order correlation which had certain advantages over the method in use; but, as it had also certain disadvantages and was not received with any show of enthusiasm, I let it drop. In other instances, I do not have so good an excuse for letting my work go unpublished.

When the War reached America, my strong inclination was to respond to the call for psychologists in the Army testing service, but conditions seemed to demand that I be the one to stay at home and carry on some semblance of psychological instruction in the University. The American Psychological Association entrusted me with the duty of seeking a test for emotional stability. The experience of other armies had shown that liability to "shell shock" or war neurosis was a handicap almost as serious as low intelligence. After considering other possible emotion tests, I concluded that the best immediate lead lay in the early symptoms of neurotic tendency which the neurologists and psychiatrists were finding in the case histories of neurotic subjects. Collecting hundreds of such symptoms from reported case histories, I threw them into the form of a questionnaire which could be applied to a group of subjects at a time, the single questions to be answered Yes or No. I tried this questionnaire on normal groups, and eliminated questions, or so-called symptoms, which were reported so frequently by the normal subjects that they could scarcely have any diagnostic value. The abridged questionnaire was tried on a thousand recruits in one of the camps, and on small groups of diagnosed abnormal subjects, and the results worked up again and submitted to a conference assembled by the Surgeon General to advise him as to the military use of the questionnaire. The decision was to give the device a trial as part of the psychological examining procedure in one of the camps. Soon afterwards, the War came to a close, leaving the question unsettled as to whether or not the questionnaire would really assist in discovering the recruits who were specially susceptible to psychoneurosis. The idea was to use the quantitative score of unfavorable responses as a first indicator, to be followed up by individual examination at the psychiatrists' hands. At all stages of this work on the "Personal Data Sheet," I had valuable collaboration—that of Poffenberger in preparing the first draft, before he went into the Army, and that of Boring in securing the results from the Army samples. Hollingworth used the questionnaire on "shell shock" cases invalided home, with interesting results. Since the War, quite a number of psychologists have used the questionnaire or modified forms of it, and, though the results have never been striking, it still seems to have possibilities of usefulness.

Since the War, I have had the honor of participating in the activities of the National Research Council and also of the Social Science Research Council, and it has certainly been a liberal education to come thus into contact with leaders in the sister sciences, and with problems which call for the cooperation of workers from different disciplines. From the year that I spent at the National Research Council, I remember with special satisfaction my association with the committees on the "psychology of the highway" and on "child development," both of which undertakings are still going on, as, indeed, there is every reason why they should. It came rather as a surprise to the psychologists, when the social science group invited our Assocation to participate in their Research Council; but I have found this group entirely hospitable to psychology and hopeful of advantage to social science from association with psychology. There is no doubt in my mind that psychology is properly both a biological and a social science, and the logical meeting place of those two groups of sciences.

Of all the organized groups that I have learned to love, none is dearer than the American Psychological Association, whose annual meetings I have attended with but few exceptions since 1898. Outsiders sitting in our meetings sometimes get the impression of mutual hostility within our group, but I am sure that is a false impression. My own impression is one of fundamental solidarity, along with the freedom of discussion that comes from direct handling of the subject-matter. If I were listing the honors that have fallen to me, I should place first that of being elected President of the Association.

Of my books, the earliest was a monographic analysis of the literature on movement and the perception of movement. There was a small book on personal (not mental) hygiene, emanating from my years as a physiologist. Next in time came a much more extensive piece of work, the collaboration with Ladd in the revision of his *Elements of Physiological Psychology*. In the revision, Ladd took care of the more philosophical parts of the work, and I was responsible for nerve anatomy and physiology and for experimental psychology. *Dynamic Psychology* was a reaction to Titchener, Watson, and McDougall, and sought for a position that should be independent and yet have room for all genuine psychological efforts. It sought also to show that the study of motivation had a proper place in psychology, no matter how positivistic the science should be. More recently I have written and rewritten an elementary textbook, and, like many other textbook writers in our science, have tried to make this a scientific contribution, by clarifying my own ideas, keeping abreast of developments, and interrelating the several topics. A survey of the contemporary schools now completes the list, but I am laboring hopefully, with many interruptions, at a general book on experimental psychology, which should soon be finished, since it was started fifteen years ago! My object is here to digest the literature on as many topics as possible in experimental psychology, weighing the evidence and effecting some measure of synthesis of the established findings.

Though my ideal all along has been "investigation," and though I have been busy all along with research in an advisory capacity, I have done comparatively little investigation on my own account. Probably my bent is more towards weighing evidence and "seeing straight" than toward active enterprise. I should have liked to be a discoverer, so that anyone asking, "What did Woodworth do?" would be promptly answered, "Why, he was the man who found out" this or that. It is likely that many other psychologists have the same feeling of disillusionment. It seems as if real discoveries, on a par with those in some of the other sciences, simply were not made in psychology. As I diagnose the situation, we started thirty or forty or fifty years ago with a background of philosophical problems. These have gradually disappeared from our view, because they were not genuine psychological problems, and we are left with what seems to be a multitude of rather disconnected problems, none of them appearing as very fundamental. We are, then, passing through the stage of becoming acquainted with our subject-matter in detail and for its own sake, and there is no telling when or where discoveries of really fundamental significance may be made—probably where we least expect them.

My bogey men—the men who most irritated me, and from whose domination I was most anxious to keep free—were those who assumed to prescribe in advance what type of results a psychologist must find, and within what limits he must remain. Münsterberg was such a one, with his assertion that a scientific psychology could never envisage real life. Titchener was such a one, in insisting that all the genuine findings of psychology must consist of sensations. Watson was such a one, when he announced that introspection must not be employed, and that only motor (and glandular) activities must be discovered. I always rebelled at any such epistemological table of commandments.

The desirable principles, so it seems to me, are those that free the investigator rather than those that restrict him. My thinking on the mind-body problem has been guided, latterly at least, by some such desire for freedom. I must have entered my first psychology course, as an undergraduate, with some half-formed spiritualistic conception, for I remember the shocked resistance with which I first encountered the notion that thought was in any degree dependent on the brain. Carpenter's evidence regarding the effect on thinking of fever, old age, and blows on the head, reminding me as they did of some experiences of my own, converted me to his interactionist view, which seemed at the time very radical and gave me a feeling of daring freedom from my older theological views. Reading Lotze in the years immediately following, and Paulsen and Höffding while a student at Harvard, about shifted me over to the parallelist position. I was impressed by Höffding's words in a book which James placed in our hands:

> "What we in our inner experience become conscious of as thought, feeling and resolution, is thus represented in the material world by certain material processes of the brain, which as such are subjects to the law of the conservation of energy. . . . It is as though the same thing were said in two languages. . . . Here this hypothesis interests us as the most natural determination of the relation between physiology and psychology. These two sciences deal with the same matter seen from two different sides, and there can no more be dispute between them, than between the observer of the convex and the concave side of a curve (to make use of a simile employed by Fechner). Every phenomenon of consciousness gives occasion for a twofold inquiry. Now the psychical, now the physical, side of the phenomenon is most accessible to us." [H. Höffding: *Outlines of psychology.* (Trans. by Mary E. Lowndes.) London, 1893, pp. 65, 69.]

James, in his lecture, joined battle with this view, supporting interaction. When I asked him, after class, if he did not think that parallelism was a good guide for the investigator, leading him to the full cultivation of his own specialty, James replied that precisely that was what parallelism was not good for, since the line of scientific progress led, the rather, by way of tracing the interaction of mental and physical.

I was left with a question in my mind rather than a conclusion. Parallelism seemed to have the neater logic, but to suffer from an atmosphere of unreality. As my work in the next few years took me into both psychology and physiology, the problem remained a very genuine one for me. I came to see, to my own satisfaction at least, that the parallelism that we know is really a parallelism of sciences. It is not a parallelism of different processes, but one of different scientific descriptions of the same process. By 1908, I had reached a view of the matter that still appears sound. The parallelism is not necessarily between the psychical and the physical, but may and does occur whenever different sciences set themselves to the description of the same natural process. The different sciences will employ different techniques, and, in particular, one science will go into finer detail than the other, even as one map goes into more detail than another map of the same country. While the detailed map certainly includes much that does not appear in the comprehensive map of a larger area, it has to leave to the latter the presentation of the broad geographical relationships. Thus the same real object can be given description at two (or more) different "levels" or magnifications. The same parallelism appears between gross and microscopic anatomy, between organ physiology and cellular physiology, between geology and the physics and chemistry of the minute processes that enter into the broad geological processes.

Let me take a more concrete example. Cellular physiology reveals something of the "fundamental" or "underlying" processes that go on in the heart muscle; but, if we want to understand the heart as a pump, we must study its action in another, less minute way. Both approaches are needed, and neither makes the other superfluous.

In the same way, the psychologist describing a conditioned reflex in terms of stimulus and response, and the physiologist describing it, as far as possible, in terms of nerve currents, etc., are describing the same identical process, the physiologist in more detail, the psychologist with more breadth. The chemist would demand still finer analysis than the physiologist gives, and the sociologist might wish to include the conditioning of the individual in a still broader view than is taken by the psychologist. Parallelism, then, is not necessarily psychophysical, but occurs whenever a more detailed and a more comprehensive description of the same thing or process are undertaken.

It may be urged that the difference between a brain process and a conscious process or experience of the subject is a more radical difference than those we have brought forward. It may seem that the psychical and the physical are so absolutely unlike that they cannot possibly be the same identical process differently described.

I used to think so, but have concluded that I had no real reason for thinking so. Without arguing the case here, I will merely point to the undoubted fact that the experience of hearing a tone, for example, belongs to a whole process, while a physiological description would certainly go into detail. If a subject under suitable acoustic stimulation reports hearing a tone, and a physiologist inspecting his brain reports certain detailed processes as occurring, there is no longer any doubt in my mind that these detailed processes are parts of the identical total process which the subject himself reports.

I should be inclined to urge that this "levels of description" theory is not quite the same as the two-language or double-aspect theory with which it started. The theory to which I have become attached (1) is not limited to the psychophysical situation, (2) is not limited to just two aspects, and (3) purports to give some account of what the difference is between the several aspects, viz., a difference in degree of breadth (or of detail) of observation and description.

The value of the theory, to me, is that it keeps all of psychology, introspective and behavioristic, within the bounds of natural science. The hearing of a tone is no less an event in the stream of natural events than the movement of the arm, or than the physiological processes into which either of these total processes may be analyzed.

The theory has a similar value in saving for psychology (and for the social sciences as well) a place to fill in the general framework of natural science. Once and again I have heard it predicted that psychology would pass away in proportion as physiology developed. The finer analysis of physiology would make the coarser psychological descriptions superfluous. Psychology, it seemed, had no ultimates of its own, and could be completely resolved into either physiology or thin air. The analogy with cellular and organ physiology comes to our rescue here, showing that the finer analysis cannot do the work of the coarser.

As to "ultimates," I look at the matter this way. No matter how completely you describe the cells composing the heart and their activities, you need to take account of the structure of the whole heart in order to tell how the organ behaves. The total structure is an ultimate in describing the action of the organ. Let me add a geometrical illustration. A ring composed of two concentric circles seems merely a derived form, and all its properties can be deduced from the properties of the circle. Yes—granted the definition of the ring as composed of two concentric circles with specified radii. This structure of the ring must be given; it is an ultimate, not to be deduced from the geometry of the circle. For psychology, the ultimates are not the electron and proton, but the individual and the fundamental types of activity determined by the organization of the individual and by the situations in which he is placed.

Such general considerations cannot be expected to serve the investigator as a guide, but they may free him from inhibitions and from a sense of futility. If I were advising a young investigator, from the standpoint of my forty years of psychology, I might point out this or that promising topic for study, but I should be more likely to tell him that the whole field was still new and open before him. He need not despise what has already been done, for it affords a much better first-hand acquaintance with the field for investigation than was available forty years ago. But many incisive discoveries remain to be made. For getting on the trail of what will prove to be important and fundamental, there is no sure rule to be given; but the experience of investigators in many fields does seem to show that persistent following up of what is queer and out of line with accepted beliefs often leads to significant discoveries. What we seem to need in psychology is surprises; and by following up a small surprise one may find a greater one beyond. Or one may not—such are the chances of the game.

ROBERT MEARNS YERKES

PSYCHOBIOLOGIST

I continue to think of the surroundings into which on May 26, 1876, I came as a first-born child, as nearly ideal. It was in the midst of a beautiful agricultural country, inhabited by intelligent, self-respecting, law-abiding, prosperous folk; hills and vales, forests and streams, as scenes of the ceaseless and ever-varying activities on a large farm, with its rotation of crops, dairying, and woodcraft. There were domestic animals of many kinds, and many laborers and mechanics came and went. The great city of Philadelphia was so near that our farm and dairy products were hauled to it overnight in horse-drawn wagons. This is the picture that appears when I think of my childhood. If I were choosing now, I should not change that environment.

Pleasant occupations abounded. Fishing, swimming, skating, berry and nut gathering, fetching the cows, learning to care for, saddle and harness, ride and drive horses, and, finally, to do, and in many instances to enjoy doing well, the multitude of things necessary to comfort and prosperity on a large farm in eastern Pennsylvania, late in the nineteenth century, filled my days and rendered them joyous.

Dominant among the recollections of childhood are out-of-door amusements; free, unrestricted, unaided study and enjoyment of nature; the care of household and farm pets; the capture and taming of wild animals. When the household cat one day killed a pet albino rabbit, I was so inconsolable that my parents had the skin mounted and thus I long kept it as a cherished possession. I was extremely fond of every sort of game, from parchesi, dominoes, checkers, and cards indoors to such rough outdoor sports as shinny, baseball, and football. Warm, after nearly fifty years, are my memories of gathering tortoise and snake eggs in new lands when first plowed, of carrying them home in my hat, preparing earth-filled boxes, "planting" them, and watching for the hatch. The young snakes usually managed to escape me, but the tortoises became treasures of entertainment. Thus happily passed the first eight years of my life.

I have delightful recollections of three of my great-grandparents, and I enjoyed and richly profited by long-continued acquaintances with all of my grandparents.[1] My parents, who belonged to families long resident in the vicinity of Philadelphia and devoted almost without exception to agriculture, lived to see me established familially and professionally. Both were ambitious, energetic, musical, religious by nature and training. My mother, a woman of rare sweetness of disposition and unusual ability, beloved of all who knew her, was the strongest influence in my early life, and I think also the wisest. My father and I were intimate merely because of blood and social relationship. We had little in common intellectually, and more often than not we disagreed in practical matters. Except for my attachment to my mother and the influence of a capable, level-headed young German then in the employ of my father, I probably should have run away from home before my fourteenth year. Lest this should appear to belittle my father, I hasten to add that I have been described as a moody, strong-willed, unsuggestible child, difficult to control. Father doubtless lacked the magic touch of sympathetic insight. In early childhood I feared him; later, I actively disliked and disapproved; and finally, in maturity, I came to pity him for characteristics which rendered his life relatively unhappy and unsuccessful.

As I write I am reminded of many incidents of family life which are illuminating. Only a few may appropriately find place in this professional sketch, and as it happens those which I have chosen refer rather to my grandfathers than to my father or self.

Grandfather Yerkes once told me, and he was then more than sixty years of age, that he did not know what it meant to feel tired! The members of his household used to say that, on arising to his day's work about four o'clock in the morning, he would loudly call the poultry to breakfast in order thoroughly to arouse the family and get things started. Father also was like that, and I should confess that in my own household I am sometimes called the slave driver.

Lack of sympathy with my father and our temperamental incompatibility very definitely turned me against his occupation and his vocational plans and desires for me. These misfortunes also robbed me of much that should be most precious in paternal companionship, training, and guidance. The following incident, taken from my relations with Grandfather Yerkes, partially explains my estrangement from Father, for his treatment of me was as direct and unsuited to my disposition as was that I would now describe. I had been set an irksome, arduous, farm task which I performed as I thought proper and necessary, but with maximum economy of effort and simplicity of procedure! Subsequently, I learned that Grandfather had complained to Father that the work might better have been left undone. I bitterly resented the criticism, which I considered unjust, but even more the fact that Grandfather spoke to Father instead of to me. Perhaps, had he come to me and tactfully explained why my method was unsatisfactory, I should have been the wiser and he respected instead of disliked.

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[1] Yerkes and Carr (paternal); Carrell and Addis (maternal).

Radically different are my memories of Grandfather Carrell, for he genuinely sympathized with my intellectual interests and aspirations and always was ready to encourage and aid me in my educational efforts.

During childhood I was much alone. A sister some four years my junior, to whom I became devoted, died when she was three, and I barely recovered from the same dreaded scarlet fever. Two brothers and another sister, born later, were so much younger that I looked upon them as charges rather than playmates.

Because of its far-reaching influence on my physical and intellectual development and my vocational choice, the scarlet fever tragedy should be more fully described. A man, prematurely discharged from a Philadelphia hospital or for other reasons a carrier of infection, came to us as a farm laborer. He was friendly with us children and, from his arrival, we were much with him. When we became ill he disappeared, doubtless conscience-stricken or fearful of responsibility. Many weeks later I learned that my little sister had gone from us. Vivid is my memory of Mother's gentle, sad words as she told me of this when, for the first time, I sat up beside a favorite window in the sunshine of early spring. In my young life that loss was irreparable. No one ever took the place of my infant sister and I continue to think of her as the most beautiful and altogether lovable of children.

The family physician, during this fight with the forces of destruction, was a cousin, Dr. John Beans Carrell, whose ministrations, often bitterly resented and opposed by my feverish self, nevertheless made lasting impressions and deeply stirred my admiration and vocational hero-worship. Ever since, in my daydreams, I have imagined myself as physician, surgeon, or, in other guise, alleviator of human suffering. This is the first indication of a social-mindedness which subsequently came to pervade my life and to establish fellow service as its chief objective.

I am wholly unable to confirm the observation, but Dr. Carrell assures me that my disposition radically changed during my grave and prolonged illness. Before it, according to him, I had been wilful, violent-tempered, obstinate, unruly, disagreeable. Thereafter I was so greatly improved as to be fit to live with! Be this as it may, I am convinced that my illness so far conditioned my physique and interests as practically to determine vocational choice.

Mine was not a home for formal educational regimen. Neither my parents nor any among my immediate relatives were college graduates. I can recall no thirst for knowledge in early childhood, and, although from six to twelve years I was passionately fond of being read to, I read little myself. Vocational imaginings came early, and, after transient longings for the delights of old-iron collector, huckster, locomotive engineer, preacher, I turned, as intimated above, to medicine mixing, and for many years purposed to become a physician. My motives I suspect were chiefly utilitarian, for the physician's life appealed to me as less harshly laborious, more interesting, exciting, heroic, useful, and altogether profitable than that of the farmer.

In my eighth year, when first sent to school, I was unable to read well and so shy that I went unwillingly and with intense discomfort until I had become accustomed to the routine and made acquaintances. For some seven years I attended the nearby ungraded rural public schools. I worked hard in school because I liked to succeed and stand well in the class. Ambition and social prestige evidently were primarily motivational, but usually I also liked the work itself and did it eagerly and without pressure in school or home. Subjects which induced lasting attitudes were spelling, because difficult and irksome; arithmetic and algebra, because I found them stimulating, interesting, game-like—their problems fascinated me, whereas memorizing repelled—and physiology and hygiene, because their objectives, information, and principles impressed me as peculiarly important.

I lacked gift of graphic expression, being then, as now, quite incapable of seeing or representing objects as does the naturally endowed artist. Musical ability, if present, I suppressed, for, despite my mother's eagerness to teach me and her urging and pleading, I never learned to sing or to play any instrument. Probably music would have been difficult for me, but I suspect that shyness and reluctance to try were the chief causes of my resistance. There are few things which in later years I have more deeply regretted than lack of musical education.

Probably I was prepared for high school, possibly for college except in the ancient languages, when in my fifteenth year I was sent with a cousin, Leonard Slack, to the State Normal School at West Chester, Pennsylvania. This was my first experience away from home and my educational baptism. I worked hard and achieved special commendation and promotions in mathematics. The fact reminds me that subsequently in college a professor of mathematics suggested that I devote myself to the subject professionally. During the year at West Chester I recall being asked by my father whether I still wished to study medicine. My reply was an emphatic affirmative. Father, as I knew, hoped that I would follow agriculture, but, if I would choose a learned profession, he preferred that it be the law. Mother, on the contrary, wished me to enter the church. Almost certainly she would have become a foreign missionary had she been free to choose a career.

I think it was about this time in my educational history that an incident occurred which fixed itself permanently in my memory. Its significance is clear. An aunt, mindful of my exceptional educational opportunities, one day asked me some geographical and historical questions. When I admitted ignorance, she expressed surprise at the imperfection of my education. I well remember my mingled feelings of chagrin, resentment, and disapproval, for her conception of education struck me as unsatisfactory. Even then my interest centered in constructive, creative effort toward the extension of knowledge, instead of in achievement of scholarship through mere accumulation of facts. Thus early, my interest in research manifested itself. The incident suggests the query: Is it perhaps true that persons of exceptionally retentive memories tend to become encyclopedically learned, whereas those of relatively poor memories, among whom I undoubtedly should number myself, tend rather to become inventive, inquiring, and constructive? Whether, in such case, psychological traits are primarily conditions or results is the question in point.

So it happened that at the age of sixteen I possessed vocational orientation and determination to obtain the educational preparation desirable for the profession of medicine. Undoubtedly, our family physician, Dr. Carrell, was chiefly responsible for this choice. His personality and professional example had stirred my imagination, and his interest, encouragement, suggestions, and advice provided the necessary basis for definite decision. Except for the happening now to be narrated, I almost certainly would have gone to Dr. Carrell "to read medicine" and thereafter have matriculated, probably without collegiate training, in his medical alma mater, the Jefferson Medical College of Philadelphia.

But things happened otherwise and thus. When I returned to the farm from my few months at Normal School, ways and means were not discernible for the continuation of my studies. Father was struggling to pay heavy indebtedness on his farm and there were three younger children to provide for. It was then that an uncle, Dr. Edward Atkinson Krusen, who had married one of my mother's sisters and recently established himself as a homeopathic physician in Collegeville, Pennsylvania, the seat of Ursinus College, offered me opportunity to earn my way in college by doing the chores about his place. There was neither doubt nor hesitation on my part, and I rejoiced greatly in my parents' consent to the arrangement.

In the fall of 1892 I entered Ursinus Academy, and after a year's preparatory work, with concentration on the ancient languages, I was admitted to the collegiate department of the institution. I elected the chemical-biological program of study, and, in addition, did preparatory medical work in human anatomy and physiology. But it was all work and no play, for my eagerness to progress held me to my academic tasks, and my duties in the Krusen home required all my spare hours, morning, evening, and Saturday. As I look back on those happy, toilsome years, it seems as though they would have been perfect if I could have afforded and arranged to have had Saturday as holiday. Yet I was far from self-pity, and I always have considered myself fortunate in my opportunity to obtain collegiate training.

After entering Ursinus I was at home only for short visits or for a few weeks during the summer harvest season, when I worked as a paid laborer. From the small savings of my youth, which, on Dr. Carrell's advice, had been well invested, and from my current earnings in the Krusen home, I was able to pay all of my expenses in college. In addition to board and room, after my first year in his home my uncle paid me a wage of ten dollars per month. This I considered generous and just. Indeed, to Uncle Doctor, as I always called him, my debt is incalculable. He was a wise, broad-minded, generous gentleman, a beloved physician, and a staunch, dependable friend. Had he been my father, and, practically, from my sixteenth to my twenty-first years he stood in *loco parentis,* he could not well have done more for me. Disinterestedly, devotedly, affectionately, he advised, guided, and encouraged me. I cannot do less than thus acknowledge my debt of gratitude and love.

A word further on personal influences. Up to the time of my entrance into college, my character, vocational leanings, educational endeavors and ambitions, had been markedly affected by six persons: my father and mother; the German farm laborer, Adolph Weise; my public school teacher, Miss Eva Roberts; and the physicians, Drs. Carrell and Krusen.

My father, I suspect, most strongly influenced me negatively. I desired to become what he was not: had he wished me to become a physician, doubtless I should have refused. My mother, on the contrary, through affection, tactful suggestion, the inculcation of the moral code, principles of character of the Christian religion and of her community, influenced me profoundly and permanently. Father's employee, Adolph Weise, was my intimate, wise friend and counselor in those years of early adolescence when I sorely needed guidance and stabilization. He read to me, talked with me of many things, aided with my lessons, and reasoned with me on endless practical matters. By sheer simplicity and convincingness of argument, this strong, clear-minded young German reasoned me away from the undesirable. Of swearing, which he abjured, although most of our farm laborers were adepts, he always said: "It is a foolish, useless, disagreeable habit. Don't form it." As I could not meet his arguments, I naturally followed his example in this and many other matters. My first public school teacher, Eva Roberts, later for many years a highly successful and esteemed teacher in Girard College, Philadelphia, deeply impressed and influenced me by her strength of character and purpose, mastery of pedagogical method, soundness of judgment, and utter justice in the treatment of pupils. I admired her almost worshipfully. There was also the all-pervasive and continuing influence of my cousin, Dr. Carrell, which certainly initially determined and confirmed my choice of medicine as a career; and, finally, that of my uncle, Dr. Krusen. To these few I owe my life and its main traits and trends. My heart goes out to them now in gratitude and affection. Would they were all here to receive such reward of appreciation as I can offer.

In June, 1897, I was graduated from Ursinus College after four profitable years of strenuous intellectual work. Two Ursinus teachers profoundly influenced me. Colonel Vernon Ruby, Professor of English, more, perhaps, than anyone else, taught me the importance of careful, thorough, honest work. My ability to use my mother tongue I owe principally to him and to the subsequent practice which his precept and example encouraged. Dr. P. Calvin Mensch, biologist, I worked with as pupil, disciple, and friend. His ideals and his enthusiasm for creative endeavor became mine. Probably my debt to him is greater than to any other teacher.

Completion of work at Ursinus found me at a crossroads, for a *deus ex machina* had unexpectedly appeared and I was offered the loan of one thousand dollars for graduate work in Harvard University. Choice was between the study of medicine in Philadelphia or the unexcelled opportunities for graduate work in biology, psychology, and philosophy at Harvard. It was a momentous decision which, as now appears, determined the course of my professional career. I was just twenty-one. Readily I convinced myself that I was young to enter medical school and might better devote at least a year to special work in Harvard before completing my medical training. It was my earnest desire to work with pre-eminently able investigators and teachers.

So, in the fall of 1897 I entered Harvard, not as a graduate student, but with provisional undergraduate classification and opportunity to demonstrate preparedness for professional work. At the end of the first year I was awarded the A.B. degree and given graduate status. I might then naturally have turned to medical studies, but instead I leaned toward preparation for research in some department of biology. Encouraged by my teachers and aided by appointments to assistantship and scholarship, I decided finally to become a candidate for the degree of doctor of philosophy instead of doctor of medicine.

Again a crossroads which compelled important decision. I was keenly interested in zoölogy and also in psychology. At the suggestion of Josiah Royce, to whom I had gone for advice when I first arrived in Cambridge in 1897, and who became my teacher, friend, and colleague, I undertook to combine these interests by devoting myself to what was then called comparative psychology. Introduced and recommended by Professor Royce, I consulted with Professor Münsterberg about opportunities in animal psychology. He was encouraging and the outcome was my transfer in 1899 from the laboratories of zoölogy, where I had enjoyed the rare privilege of working with E. L. Mark, G. H. Parker, C. B. Davenport, and W. E. Castle, to the laboratory of psychology, in which, during the succeeding eighteen years, as student, assistant, instructor, or professor, I conducted psychobiological research and instructional courses in comparative and genetic psychology.

From the beginning of our acquaintance, Hugo Münsterberg, with almost paternal interest and solicitude, and with rare generosity, aided me both professionally and personally, and, although I never was able to admire him as scientist, I learned to prize highly his friendship, enthusiasm for research, and scholarship. Throughout our association from 1899 until his death in 1916 our relations were intimate, and I was constantly the beneficiary because of his learning, extensive professional acquaintance and knowledge of the world, and his devotion to research. I seriously doubt whether I should have remained in Harvard more than one or two years except for his influence and encouragement. Thus I acknowledge a great debt.

In 1902 I was granted the doctorate of philosophy in psychology and offered an instructorship in comparative psychology in the University, with half time for research and a salary of one thousand dollars per year. I well remember Professor Münsterberg's friendly question when he told me of the opportunity: "Can you afford to accept it, Yerkes?" "No," I replied, "but I shall, nevertheless." Thus began a period of professional service to Harvard University and science which continued until it was interrupted by the World War in 1917.

During those fifteen happy, eventful, fruitful years of research and teaching I gave my best to Harvard and received incomparably more benefits from rare associations and companionships than I could give in return. It was for me a period of intellectual and cultural growth and enlightenment, of constant stimulation to improvement and achievement, and of precious inspirational influence. For, unworthily, as it seemed to me, I was a member of a university faculty group of pre-eminently great scholars and great personalities, which at one time or another during the period in question included Josiah Royce, George Herbert Palmer, William James, Hugo Münsterberg, Francis Peabody, George Santayana, Dickinson Miller, Robert MacDougall, Edwin B. Holt, and Ralph Barton Perry. These, my colleagues in the Division, which was then inclusive of philosophy, social ethics, and psychology, were men of such personal quality, originality, and creativeness, as seldom are found in an academic group.

Thus, with victory for the latter, ended in 1902 the struggle between medicine and psychobiology in my vocational imaginings. My taste for scientific research, if not my ability, had long before been revealed at Ursinus when my teacher and master, Professor Mensch, himself a doctor of medicine and of philosophy, proposed for my training the investigation of a problem in physiological chemistry. I did not solve the problem, but in the attempt I learned much about myself and the attractiveness of biological research. From that time I knew positively that I wished to give my life to constructive work in the biological sciences rather than to practical service in medicine or surgery. It was then that I first resolved that making a living should, so far as practicable, be merely incidental to my life work. And so, as it turns out, it has been, these thirty years! But when I abandoned the study of medicine, lively interest in its varied problems and in the sciences basic to both medicine and surgery persisted. Although I lack a medical degree, my dominant interests classify me with the profession. Much of my work has been conducted in medical institutions; more might have been, and my friendships and companionships continue to bear witness to my natural taste and my initial vocational leaning and choice. All this merely to establish the fact that in reality my original choice of career was modified, not abandoned, and my professional interest broadened and liberalized instead of turned into unrelated channels.

A plan, whose realization after nearly thirty years has now been nearly achieved in Yale University, came to me as a stirring vision of usefulness during my graduate days in Harvard. It was the establishment and development of an institute of comparative psychobiology in which the resources of the various natural sciences should be used effectively for the solution of varied problems of life. Naturally, psychological and physiological interests dominated in this vision. For a time it seemed that the dream might speedily come true in Harvard, but President Eliot, wise and far-sighted promoter of productive scholarship and of medical education and research, retired from his responsibilities just too soon. Instead of receiving encouragement in such seemingly impractical planning as I had been indulging in, I was gently and tactfully advised by the new administration that educational psychology offered a broader and more direct path to a professorship and to increased academic usefulness than did my special field of comparative psychology, and that I might well consider effecting a change. In disregarding this well-meant and wholly reasonable advice, I ran true to form. To do what I had especially prepared myself for, what I felt pre-eminently fitted for, and what, above everything else, I wished to do, seemed to me incomparably more important and desirable than a professorship at Harvard. Several of my professional colleagues agreed with me. Many times since I have had to confirm that decision or to make similar ones. I never have regretted the abiding determination to live my own professional life, irrespective of administrative and other practical considerations.

During the first year of my Harvard instructorship, opportunity appeared for a brief visit to Germany and Switzerland to study the organization and equipment of physiological and psychological institutes. This was in preparation for the planning of suitable building and facilities for comparative and other branches of psychological work in Harvard. The experience naturally was very valuable to me. It was seventeen years before I again visited Europe, and then it was to England and France that I journeyed. This neglect of international professional contacts was due to financial limitations and the demands of my research, not to lack of interest or desire. Indeed, it has proved a very serious disadvantage. As I write these words, I am on my third professional foreign tour, which includes visitation of numerous psychobiological establishments in the principal countries of Europe, and, in addition, the laboratories of the Pasteur Institute at Kindia, French Guinea.

In 1905, when I was fairly started in my career as a psychobiologist, began a partnership with Ada Watterson (Yerkes), which perfectly blended our lives and incalculably increased our professional and social usefulness. Successful marriages appear in these times to be not unworthy of record and remark. Moreover, from 1905 my professional autobiography is no longer mine alone. At this moment our partnership is publishing jointly, as the outcome of six years of continuous preparatory labor, a book on anthropoid life, *The Great Apes*.

Crowded with interesting activities were the years between 1902, the beginning of my professional life, and America's entrance into the World War. My intellectual environment was stimulating, conditions within and without were favorable to creative endeavor, incentives to service abounded. I was busy, contented, happy in my scientific work, my family life, and friendships. As my colleague Ernest E. Southard once remarked, professionally speaking, for years I lived on cream. To supplement my small and obviously insufficient Harvard salary, which during fifteen years of service as instructor and assistant professor averaged about two thousand dollars a year, I taught in Radcliffe College, Harvard Summer School, and the University Extension Department in Boston. It was through my teaching of elementary psychology that I first was brought into contact with Edward B. Titchener. Use of his textbooks in my courses provoked exchange of opinions, discussion, and, on my part, endless questions, for in introspective method and its results I was the novice, he the master. I treasure a folder of letters which represent much of my vital exchange with the most learned psychologist I have ever known. Whatever interest I have in introspection, competence in its use, and appreciation of its results, and whatever I know of the psychology of the self, as contrasted with objective psychology, I owe primarily to Titchener. With his aid I came to distinguish sharply between my special interest in the materials and problems of psychobiology and psychology as the science of experience. Efforts to systematize my thinking in this direction for the benefit of my students resulted in the publication of my *Introduction to Psychology,* the first and only textbook I have had the will to write. My professional debt to Titchener is equaled only by that to Münsterberg, Royce, and Holt.

In the midst of intensive work with students and colleagues in the Harvard Psychological Laboratory I found time for several profitable adventures in cooperation. These are some of them. From association as pupil and assistant with Edward L. Thorndike at the Marine Biological Laboratory at Woods Hole, I profited much. Later for some years I labored with John B. Watson for the improvement and the standardization of methods for the comparative study of vision in animals. At this time Watson was in Baltimore, I in Cambridge, and our exchanges were mostly by letter. One spring, to my great satisfaction, I was granted leave of absence from Harvard to acquire knowledge of neuro-surgical technique through association with that skillful technician and brilliant investigator, Professor Harvey Cushing. My weeks in the Hunterian Laboratory of the Johns Hopkins University, under the guidance of Cushing, provided stimulating, enlightening, and revealing experience, whose effects were permanent. In addition to technical training and new professional insight, I carried from the laboratories of comparative surgery an enduring friendship. That I have made no noteworthy contributions to neurology or psychobiology by way of surgical techniques is the fault of circumstances beyond my control. Yet another important season was that spent with my former pupil, Gilbert V. Hamilton, in his ideally situated private laboratory at Santa Barbara, California. And with another pupil, Daniel W. La Rue, who, like Hamilton, returned with interest what little I had been able to give as teacher, I planned, used in courses of instruction, and finally published *An Outline of the Study of the Self.*

Much more than an episode in my almost too full professional life of the young century was opportunity, on recommendation of Ernest E. Southard, Professor of Neuropathology in the Harvard Medical School, and Scientific Director of the Psychopathic Department of the Boston State Hospital, to serve as psychologist in the Hospital. This was my introduction to research in psychopathology. During five years I gave half of my time to the direction of psychological service and research in the Hospital. It was here I discovered certain urgent needs of psychiatry for improved techniques of psychobiological examining and measurement, and here also, with the aid of graduate students and assistants, I developed the point-scale method of measuring aspects of intellectual activity and the multiple-choice method for the study of ideational behavior. Naturally, both practical and theoretical relations of psychobiology to medicine, and more particularly to psychopathology and psychiatry, commanded my attention and I thought and talked much about ways of rendering these subjects more helpful to one another.

I have mentioned Ernest Southard as my master in psychopathology. He was that and much more, for, even after a decade of separation from his influence, his brilliant originality, vision, versatility, and tireless industry, continue to stir my imagination and to spur me to more fruitful effort. His was a remarkable intellect, backed by exceptional training and vision, which neurology and psychiatry could ill afford to lose either early or late.[2]

Those were particularly stirring years, for when I accepted hospital duties I gave up no portion of my teaching burden or program of research in the Cambridge laboratories. Doubtless, it was fortunate for my health that in its fifth year this dual life was abruptly ended by the World War. The internal values of my concentrated practical experience in psychopathology it would be difficult to overestimate. The external results are scant because I published relatively little.

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[2] Doctor Southard died February 8, 1920.

Throughout my Harvard connection several graduate students each year shared my labors and enthusiasm for discovery and invention. I then considered the university the logical and altogether fitting home for research, and I now even more strongly hold that conviction after some thirty years of varied professional experience, both within and without American universities.

Stable in my professional life and not over-eager for increased income or rank, the current ran smoothly and it seemed that I might continue at Harvard until the end of the chapter. It had been relatively easy to refuse numerous opportunities to migrate. Then out of the war-clouded sky came an attention-compelling, insistent call to reorganize psychological work and take direction of the laboratory in the University of Minnesota. At first I declined thoughtfully and reluctantly, with the urgent advice of Professors Royce and Münsterberg. But when, a year later, the offer was made even more alluring, I hesitated and was lost to my university birthplace and home. It was a difficult decision, opposed I recall by such disinterested friends and advisers as Professors Royce and Herbert W. Rand, but supported by such as ex-President Eliot and Professors Münsterberg and Taussig.

I was in my fortieth year when, in the spring of 1917, I accepted the Minnesota appointment. Barely had I made this new arrangement than America's entrance into the War upset all of my plans. For two years after resigning my appointments in Harvard and in the Boston Psychopathic Hospital I held my western academic post and during that time made necessary recommendations for staff reorganization, planned the establishment of a department of psychology, and arranged for the transfer of the laboratory to a new site and building. It was a profitable experience, although in the end I resigned my post without having at any time been resident in Minneapolis. For this circumstance the War was wholly responsible. The members of my staff in Minnesota who, after my resignation, carried on effectively included, in addition to Herbert Woodrow, who was originally on the ground, Richard M. Elliott, William S. Foster, Mabel Fernald, and Karl S. Lashley. A better-trained, more able, and altogether competent group of young psychologists was not to be found.

Thus, with America's declaration of war ended one of the most important periods of my professional career—measured by twenty years as student, teacher, and investigator in Harvard University. It is appropriate to note here the distinctive characteristics of my research interests and results during this period.

My first scientific paper was published from the Laboratory of Comparative Zoölogy of Harvard in 1899, when I was twenty-three years of age. It was the outcome of suggestions received from my teacher, Charles B. Davenport, and of observations made under his direction. The title of this maiden publication in psychobiology, *Reaction of Entomostraca to Stimulation by Light,* indicates one of my major fields of interest, namely, organic receptivity, its nature, conditions, and relations to behavioral expression and to experience. There followed several papers on phases of receptivity and response in invertebrates. All show the helpful influence of my biological teachers, Messrs. Mark, Parker, and Davenport, and all are classifiable under the physiology of the nervous system, although even then it would have been fairer to my interest and point of view to place them in psychobiology.

Shortly my interest extended to include organic adaptivity, which then was almost universally designated as habit formation, and from 1905 to 1912 I published several reports of investigations on adaptivity and receptivity in such relatively lowly vertebrates as the amphibians and reptiles. Other aspects of physiological process which at this time suggested to me important neurological problems were temporal relations of response, inhibition, and facilitation. A little later I became profoundly interested in problems of instinct versus individual acquisition, and several of my investigations and those conducted under my direction were concerned with the essential characteristics and relations of maturational or so-called hereditary modes of response and their neuromuscular mechanisms.

I still consider solution of the assemblage of problems suggested by these phrases of the utmost theoretical and practical importance. Many times my work on the mechanisms and behavioral expressions of inheritance and acquisition has been interrupted, once by the loss of my colony of dancing mice, and again by the World War, which found me with apparatus ready for continuation of work with mice. Investigation of the behavior of wildness and savageness in rats, well begun with the cooperation of Professor William E. Castle, I abandoned because conditions of experimentation were not favorable to reliable results.

Especially conspicuous in my research has been interest in methods and efforts to advance comparative psychobiology by invention, adaptation, and improvement thereof. My work, I suspect, has been characterized rather by ingenuity and originality than by technical skill and mechanical gift. Theoretically, method conditions progress; practically, it has always seemed to me more important than observation. My investigations, I think, entirely support this conviction, for the greater part of my life has been devoted to methodological work in the biological sciences.

I have mentioned my abiding interest in the problems of organic receptivity, adaptivity, and instinct. Always my research has been more nearly physiological than psychological, for I have dealt with problems of behavior, not with experience. Therefore my constant use of the descriptive term psychobiology. That either my interests or methods of work, my descriptions or interpretations, have become consistently more or less objective during the past thirty years I am not aware. Certainly there have been fluctuations of opinion, and gradually the conviction has strengthened that open-mindedness, willingness to envisage all problems and all trustworthy results, and to consider and test the value of all types of method, are prime essentials for the advancement of knowledge. With extreme objectivism, as voiced during the early years of my career by such eminent biologists as Loeb, Beer, Bethe, and von Uexküll, I have never been able to sympathize unreservedly because it impressed me as dangerous in its restrictions and negations. On similar grounds I have rejected the more recent objectivism, or as he calls it, behaviorism, of Watson, for it is characterized by the same logical and practical defects which appear in the historical types of psychological objectivism. More forcibly than ever, after thirty years of earnest thought and persistent study of problems of organic behavior and experience, it strikes me as wholly indefensible, and extremely unprofitable, to deny the possibility of scientifically investigating phenomena of experience in their relations to other vital happenings.

That my own interest has always centered in problems of organic structure and function in no degree prejudices me against the study of consciousness and mind. Instead, I consider the problem of the nature and relations of consciousness as at once the most fascinating and the most important in biology, and it is my earnest hope that I may live to help in some measure toward its solution. That my path is not obviously directed toward this end needs neither explanation nor apology. My course in research is pragmatic.

The scope of my research was broadened in 1913 by the addition of psychopathology, for it was in that year I accepted appointment in the Boston Psychopathic Hospital. Naturally, I undertook work in psychotechnology which promised to be helpful to psychiatry, but at the same time I formulated and, with my peculiar equipment as comparative psychobiologist, attempted to solve certain problems relative to the nature and causation of psychobiological disturbances and defects. Unwittingly I was thus prepared for the military opportunities and demands which were shortly to confront me. Had I planned my adventure in practical mental measurement with full knowledge of what awaited me in the World War I could not have arranged things better. My work at the Hospital was abruptly terminated by the War, but, even without it, removal to the University of Minnesota would have caused a break. Much of my work in psychopathology continues as I then left it, unfinished.

It was thus the presidential proclamation of April, 1917, found me. At the moment a group of experimental psychologists was meeting informally at Harvard University. Naturally, we asked ourselves what professional service American psychologists might hope to render in the military emergency. Discussion revealed eagerness, coupled with optimism and assurance that some, at least, of our techniques could be made serviceable.

Because I happened to be President of the American Psychological Association, it became my privilege and duty to take the initiative in organizing our group and in attempting to discover ways in which we might be useful. It is indicative of my lifelong professional leaning and affiliations that I promptly established relations with the Medical Department of the Army and that the major service for which I was personally responsible throughout the War, the psychological examining of recruits, should have been conducted in that arm of the service.

The story of psychological service has elsewhere been told officially and completely, if not in detail.[3] It is appropriate here to consider its principal relations to my professional life.

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[3] Yerkes, R. M. Report of the Psychology Committee of the National Research Council. *Psychol. Rev.*, 1919, **26**, 83-149. Psychology in relation to the war. *Psychol. Rev.*, 1918, **25**, 85-115. The measurement and utilization of brain power in the army. *Science*, 1919, **44**, 221-226, 251-259.

Yerkes, R. M., and Yoakum, C. S. Army mental tests. New York: Holt, 1920. Pp. 303.

Cobb, M. V., and Yerkes, R. M. Intellectual and educational status of the

For nearly two years I lived in military psychology, with scarcely a thought of the psychobiological problems which previously had occupied me. The novel opportunity which my profession created for itself in the American military establishment called for constructive planning, combined with methodological resourcefulness and skill. For these demands, as contrasted with many which more usually come to the academician and investigator, it shortly appeared that I possessed unusual qualifications.

During my term of military service I wrote little for publication. There was no time. But my official correspondence was both extensive and profoundly important for my intellectual and technical growth and the development of facility in verbal expression. It was necessarily descriptive, expository, argumentative, for my chief task, aside from making clear what we planned and proposed, was to convince military and civil officials that what we desired to undertake possessed practical value. Often it seemed that my foremost duty and obligation—one for which I usually felt myself peculiarly unsuited—was to vanquish seemingly insuperable difficulties by overcoming the passive resistance of ignorance and the active opposition of jealousy, misinformation, and honest disagreement.

Fortunately, I flourished amidst difficulties and discouragements, and the service which my group rendered finally yielded abundant satisfaction. It has been characterized by those who observed it from above the battle as uniquely significant alike for military progress and for the development of psychology and its technologies. Assuredly it was highly beneficial to me to be carried by force of circumstance from the comfortably sheltered provincialism of a great university into the swirling current of world conflict. As never otherwise could have happened, I was brought into active give-and-take contact with men of varied interests, abilities, and points of view, at a time when every man rose superior to himself; with national and international problems, plans, and programs; with organizations, methods of administration, and ideals which are foreign to academic experience. Necessity made me at home in this novel situation and I was able to present

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medical profession as represented in the United States Army. *Bull. Nat. Res. Council,* 1921, **1**, 457-532.

The personnel system of the United States Army. Vol. 1. History of the personnel system; Vol. 2. The personnel manual. Published by the War Department, Washington, D. C., 1919.

Psychological examining in the United States Army. *Mem. Nat. Acad. Sci.,* 1921, **15** (Official report).

and maintain the needs, claims, and merits of my profession as determinedly, and I think also as effectively, as I could have done in my customary environment. As obligations and opportunities multiplied, so also my knowledge, insights, faith, and will to succeed, and when suddenly the great conflict ended I was so completely engrossed in helping to increase the efficiency of the military organization of my country that for a time I felt like a person without a calling.

If ever I have spoken or written as though the contribution of military psychology in Army or Navy was largely mine, I would beg here to correct the impression. Mine, as it happened, was the responsibility for initiative and leadership, but scores of my colleagues enthusiastically and loyally gave their best. To mention names would be invidious and in bad taste, because the honor roll is too long to be reproduced entire. I could have accomplished little indeed without the whole-hearted, generous, and efficient constructive work of my fellows. The reward of growth, self-revelation, and confidence in my ability to serve mankind which came to me by reason of my share in the great conflict is more than adequate compensation for the arduous labors of the most trying years of my life.

As never otherwise could have happened, military opportunities, demands, and achievements gave American psychology forward and directed impetus. Owing primarily to an endless succession of difficulties, resultant delays, and finally the termination of the War just when our service was fully organized, our methods perfected, and authority granted for the extension of our work throughout the Army, the strictly scientific as contrasted with the practical returns of our labors, although by no means unimportant, proved meager in comparison with what we had planned for and legitimately expected. It will be long, however, before our profession entirely escapes from the directive influence of psychotechnological military developments or forgets that almost incredibly extensive and precious gift of professional service, which to the laity and the military profession was the more impressive because wholly unexpected and unsolicited.

When discharged from the Army shortly after the Armistice, I found myself faced with choice between continuation of work in Washington in connection with the National Research Council, through which much of our psychological military service had been organized and rendered, or reporting for duty in the University of Minnesota. For two reasons, chiefly, I hesitated and then decided to resign my academic post: I wished to complete and superintend the publication of the official report of our psychological work during the War, and, picking up the threads of my psychological past, to endeavor to find financial support for systematic utilization of the anthropoid apes in biological research. The latter interest, as one of the most important in my professional career, here demands brief historical comment.

In the course of comparative studies of receptivity and adaptivity which I conducted or directed in Harvard University, and especially because of the work of my student, M. E. Haggerty, on imitative tendency in monkeys, and varied observations of my own on marmosets, monkeys, and orang-outans, I had become convinced that, for certain major groups of psychobiological problem to whose solution I hoped to dedicate my life, the primates, and, more particularly, the great apes, promised to be supremely and perhaps also uniquely serviceable. My conviction found expression in a plan of action which I formulated for publication as early as 1916.[4] Following the publication of this plan several offers of assistance came to me, but no one of them could be safely accepted because I was not financially independent and thus able to give my time to the project without compensation. From 1917 to 1919 my efforts to finance suitable laboratories were necessarily in abeyance, but my dream recurred with increased vividness and compelling power when the war clouds vanished. So it happened that I was ready and eager to serve the National Research Council as chairman of one of its divisions, in part because the connection enabled me to remain in Washington where conditions seemed peculiarly favorable for the promotion of my pet project.

When I originally decided to stay in Washington instead of going to the University of Minnesota, I supposed that it would be for only one or two years, for I was optimistic that within that period I should succeed in arranging to go forward with my research. But it was not so. Disappointments succeeded one another as in the Army, and the period stretched to five years before I escaped to more congenial activities. In the meantime my personal research was almost wholly in abeyance and my only noteworthy service to my particular branch of science was the organization and facilitation of research in problems having to do with aspects of sex and human migrations. This work was done primarily through the agency of committees. I initiated and for more than two years served as Chairman of the Committee on Scientific Problems of Human Migration of the National Research Council,[5] and simultaneously gave much of my time to the Chairmanship of the Council's Committee for Research in Problems of Sex. During my association with these committees we were able to secure, through the National Research Council for the support of our programs of research, sums aggregating eight hundred thousand dollars. That our promotional endeavors were fruitful is convincingly established by the content of scores of reports which have been published by cooperating investigators. Although it was far enough from my primary interest and desire, I nevertheless took great satisfaction in this promotional work, and I even dared to hope that the committee method as we developed it might become so well established as to continue in use. In this, the migrations organization proved disappointing, whereas that for the study of problems of sex has continued with increasing usefulness to the date of writing.

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[4] See "Provision for the study of monkeys and apes," *Science,* 1916, **43,** 231-234.

As I reflect on my experiences I realize that personal relations during my sojourn in Washington were far too significant professionally to be ignored. My period of military service was slightly less than two years. The National Research Council elected me to membership in 1917 and for several years I served that organization in various capacities. Among the many delightful and professionally invaluable acquaintances and friendships which came to me during seven years' residence in Washington, I mention the following because of their pre-eminently great influence on my professional career: with George E. Hale, astronomer, the boldly imaginative and constructive genius of American science; with John C. Merriam, paleontologist, wise, far-sighted organizer and director of research; with Raymond Dodge, physiological psychologist, gifted in methodological inventiveness, friendship, and loyalty; with Clarence E. McClung, zoölogist, socially minded, devoted investigator and leader in the organization of research; with Victor C. Vaughan, bacteriologist-physician, beloved and widely influential teacher, investigator, friend; with William H. Welch, pathologist, fount of wisdom, adviser of unnumbered thousands of medical students, colleagues, and friends.

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[5] Yerkes, R. M. The work of the Committee on Scientific Problems of Human Migration. *Reprint and Circular Series of the National Research Council,* 1924. No. 58.

Wissler, C. Final report of the Committee on Scientific Problems of Human Migration. *Reprint and Circular Series of the National Research Council,* 1929. No. 87.

As, earlier in life, it was my good fortune in Harvard University to be intimately associated with men of genius in scholarship and in the art of living, so somewhat later I enjoyed in Washington the incomparable advantages of working with men such as I have named, of wider and different experience, more thorough scholarship, more varied insights, and better intellects than my own. One's professional achievements may not be understood if such aspects of social environment as these are overlooked.

In the spring of 1924, seven years after I left Harvard to enter the Army, I was enabled to return to my professional career by appointment to a professorship in the Institute of Psychology of Yale University. This research position I accepted with the understanding that I should be free to devote myself to comparative psychobiology and to promote, as might prove practicable, achievement of facilities for the scientific utilization of anthropoid subjects. The agreement was for a term of five years. Although it did not provide immediately precisely the type of establishment and equipment which I had long desired and labored to bring into existence, it did supply an institutional connection which, largely because of the sympathetic interest and professional knowledge of President James R. Angell, promised to be incomparably useful.

Turning immediately from my administrative and promotional activities in the National Research Council, I devoted the summer of 1924 to anthropoid research in Havana, Cuba, where, thanks to the generosity of Señora Rosalia Abreu, and with the cooperation of the Carnegie Institution of Washington, I was able to observe a large colony of primates. On returning from Cuba, I took up residence in New Haven.

Progress has been rapid in several lines of endeavor during the five years which I have spent in Yale University. Signally important for the realization of my plans are the following achievements: (1) The establishment in New Haven of a special laboratory for psychobiological study of primates; (2) completion of an inclusive survey of the naturalistic and experimental literature of anthropoid life, preparation of an informational catalogue, abstracts, and indices, and the publication of the source book for investigators previously mentioned as *The Great Apes;* (3) supplementation of the New Haven primate laboratory by establishment near Jacksonville, Florida, of a subtropical anthropoid station in which subjects may be bred and observed; (4) perfecting of arrangements for systematic naturalistic study of the chimpanzee and gorilla in Africa; (5) preparation and publication of a program of psychobiological research with anthropoid subjects; and, finally, (6) formulation of plans for a department of comparative psychobiology in Yale University which shall include the existing primate laboratory and be conducted in conjunction with, and as the academic headquarters of, the Florida station.

Throughout this period of continuous intense activity I have endeavored to prepare the way for effective use of anthropoid apes and other primates in the solution of assemblages of problems which include the psychobiological, physiological, psychopathological, anthropological, and sociological. Always the ape has been thought of as means to an end: namely, the solution of important problems which may not readily be approached initially by aid of human subjects. Despite considerable contributions of fact, this section of my professional life may best be characterized as one of systematic preparation for work which doubtless will engage many investigators over an indefinite period.

In 1929, after fifteen years of persistent effort, the provision for anthropoid research which I first proposed and urgently recommended in 1916 finally was achieved. Above I have referred to this consummation of my efforts as the establishment of special primate or anthropoid laboratories and station. Not even the difficulties and discouragements of psychological military service equaled those which at one time or another confronted me in my attempts to secure suitable provision for study of the anthropoid apes. Visionary, impracticable, promising slight returns, too difficult of realization, impossible, are some of the unfavorable characterizations offered as objections to investment in the plan. To have succeeded after so long a period of endeavor is heartening indeed. It renews and redoubles my faith in both plans and objectives and my desire to press forward.

As I write these words (September, 1929) I am on an extended tour of those foreign laboratories whose research equipment, personnel, and publications bear obviously important relations to the psychobiological work which I have projected. I have visited several institutions and conferred with many colleagues in Europe and am now homeward bound from the African laboratories of the Pasteur Institute at Kindia, French Guinea, which some eight years ago were established for utilization of the chimpanzee and other African primates in the investigation of problems of disease. Few experiences are more inspiring than discovery or rediscovery of the fact that scientific interest, activity, and sympathetic appreciation recognize no geographical, national, or racial limitations.

My immediate work and my plans for the future find appropriate setting in the recently established Institute of Human Relations of Yale University, in which the former Institute of Psychology has been incorporated, and in the Human Welfare Center of which the Institute is an important part.[6] I firmly believe today, as ever, that comparative method and infrahuman organisms may and will be made to contribute increasingly and importantly to the solution of a multitude of pressing human problems. I believe also in the logic and fitness of establishing laboratories of comparative psychobiology in conjunction with those of physiology in a great center for research in social biology, and as supplementary to the appropriate special establishments for human psychology, psychotechnology, and the various social sciences.

Such value as this account of my professional life may have for the reader, aside from the satisfaction of his legitimate curiosity, is more likely to come from analysis and revelation of character, motives, and methods, than from simple record of achievements or failures. This assumption is my excuse for concluding with an attempt at revelation and appraisal which, if not complete and adequate, is at least honest.

Physically handicapped from my seventh year by scarlet fever, I have had to conserve my strength and act circumspectly in order to work continuously and efficiently. Probably this explains why intellectual and especially professional satisfactions have come to dominate over physical pleasures. Endowed with a mentality in many respects ordinary, I have always had the advantage of a few wholly extraordinary abilities. Love of work and the power to tap new reservoirs of energy seem to have been paternal heritages which the circumstances of my life greatly strengthened. From childhood I have been able to work easily, effectively, and joyously, even when associates whom I considered my superiors physically and intellectually faltered or failed. This I attribute more largely to exceptional planfulness, persistence, sustained interest, and abiding faith in the values of my objectives, than to unusual intellectual gifts or acquisitions. My love of planning and a degree of prophetic insight therein, which sometimes seems to approach genius, have, I suspect, more than compensated in my professional life for relatively poor memory, a degree of inaptitude for the acquisition of languages which, to the amusement of my family, I often refer to as linguistic idiocy, and almost complete lack of power of artistic expression either graphically or vocally.

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[6] Since this was written two years ago, the plan of organization has been altered. My work is now administratively a section of the Department of Physiology of the School of Medicine, Yale University, and I am in charge of the Laboratories of Comparative Psychobiology.

As I view my life in retrospect, its professional achievements, and especially its originality, constructivity, and fruitfulness, which many of my colleagues characterize as exceptional, are attributable primarily to the habit of planning with care, foresight, and acquired skill whatover I propose to undertake, to steady unflagging interest and constancy of purpose, and, finally, to persistence which is slow alike to yield to discouragement or to admit failure.

At the age of fifty-three, and though deriving from long-lived stock, I cannot say, as did my paternal grandfather in his sixties, that I have never known fatigue. Instead, it is what I most often have had to work against. Reputed among my intimate friends and my family to be a hard worker, I have never been able to accept the fact, for during most of my years of intense professional activity I have worked not more than eight in each twenty-four hours. It is true, however, that during hours of application my concentration usually is intense and my efficiency relatively high. That the chief if not the only secret of my professional progress is hard work finds illustrative support in my ability to use my native tongue. Not infrequently, when I speak to professional friends of my joy in writing, they voice either surprise or envy. I think I enjoy composition almost as much as I do inventing, planning, or perfecting apparatus and methods or the act of observation, but I cannot discover in my present measure of ability unusual native or inborn gift. To me it seems instead the product of ceaseless practice from youth to the present moment. It is said that I have published much, perhaps it might be said too much, but nevertheless of what I have written during the last thirty years I estimate that barely one-tenth has been published. Letter writing has, I am sure, immensely increased my facility in expression. If relieved of the irksomeness of making a multiplicity of symbols, I usually would rather write to a friend than eat my dinner!

Aside from the improving influence of practice in writing, I attribute my power of verbal expression to systematic use of the dictionary early and late, with resultant growth of vocabulary and increase in the precision of use of words. As a boy of twelve I carried in my pocket a handy English dictionary which I consulted on opportunity during the day's farm work. Often in later years I have wondered whose suggestion led me to this method of self-improvement.

Were I required to single out the one characteristic which, above all others, has influenced my professional career it would have to be planfulness. Whenever I have had to compete with my fellows I have succeeded, if at all, by prophetic planning rather than by greater activity or longer effort. The purity of my joy in creative effort—it may as appropriately be called play as work—probably is due chiefly to self-determination, for more often than not I have followed freely and consistently my judgments, plans, preferences, and desires, instead of another's. Whether it be a merit or a shortcoming, I am not a good follower. It cramps my dominant trait, planfulness, and reduces me to a species of intellectual slavery. The low levels in my career are due to inhibition of initiation through limitation of self-determination, and, correlatively, the high levels to large freedom for planning and achievement.

Looking backward over thirty years of diligent labor and abundant intellectual, social, and material rewards, I am impelled to view all as preparation for the future. It is as if I were now on the threshold of a great undertaking which from the first was dimly envisaged and later planned for with increasing definiteness and assurance. Whether in this characterization of my past and prophecy for my future I am substantially correct, time will reveal. As ever, I am optimistic and determined. The prospect is alluring, for, as never before, and in a measure beyond my hopes, it promises the fulfilment of my persistent dream for the progress of comparative psychobiology and the enhancement of its values to mankind through the wise utilization of anthropoid apes and other primates as subjects of experimental inquiry.

My professional self and the program of research which has become identified with that self are parts of a movement which will dominate the twentieth century, the socializing of biology. In this great movement, as in the problems which must be solved and the practical services rendered for its facilitation, I am single-mindedly and intensely interested. As a rule remote or inclusive objectives are hidden or obscured by a multiplicity of immediate demands and responsibilities. Therefore, I have presumed to point a goal toward which all mankind is struggling and to claim it as my own. It should not be difficult to merge the self with such a goal or to lose one's life completely in its quest.

It is ungracious to preach to one's professional colleagues. Here they should stop. Only those whose careers are in prospect may safely continue! The wisdom which has come to me from vicissitudes and achievements finds expression thus: to recognize and accept one's limitations cheerfully, bravely, but also intelligently; to choose as vocation, and to render service through, work for which one is well fitted by nature and acquisition, and, in so doing, to utilize one's special abilities to the utmost. This is the best recipe I have discovered for social usefulness and for personal happiness.

I have done scant justice to my creditors in this brief human document. What throughout I have referred to as such actually is not mine. More truly and largely it belongs to those whose work throughout the ages prepared the way for my constructive efforts and to those also who have labored for and with me as teachers, pupils, assistants, colleagues. In contemplation of my debts, I stand humble and reluctant to use the personal pronoun, for the professional strivings and achievements which I have recorded are ours and thine even more than mine. This is my inadequate acknowledgment to those who have gone before and to those who have personally companioned, guided, enlightened, and inspired me.

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